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Lane, B. H., 1935, Suggestions to authors of papers submitted for publication by the United States Geological Survey: U. S. Geol. Survey, Washington, D. C. (For sale by Superintendent of Documents, Washington, D. C. 35c.)

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COMMENTS ON STRATIGRAPHIC PROCEDURE AND TERMINOLOGY

W. J. ARKELL

ABSTRACT. Comments arise out of an analysis of procedure and terminology in stratigraphy recently published by Hedberg and more especially criticisms thereof since published by Schindewolf. The terms formation, zone, stage, age, correlation, are clarified. A comparison is made between procedure in stratigraphic classification in the earliest days of William Smith and Oppel and at the present time, and there is found to have been no essential change. A plea is made that stratigraphic nomenclature should be recognized as a separate subject or discipline, with its own rules, which should regularize traditional practice in typography, ban changes of name, and standardize a small number of clearly defined terms.

INTRODUCTION

There has recently been published (Hedberg, 1954) a comprehensive analysis of "procedure and terminology in stratigraphic classification," with a useful abstract of the "Report of the American Commission on Stratigraphic Nomenclature," and also a stimulating criticism of it from the point of view of a distinguished individual German (Schindewolf, 1954). Both these works were received by me too late for consideration in a summary of modern principles and practice of classification and correlation as applied to the Jurassic System of the world, which was already in proof (Arkell, April 1956). I do not find that either work shakes me in any of my interpretations, usages, or conclusions, but I feel strongly that a number of matters raised by Dr. Hedberg and Prof. Schindewolf, and some others, call for further discussion and elucidation before the 1956 session of the International Geological Congress in Mexico. The object of this paper is to clarify the principal terms under discussion and to offer miscellaneous relevant comment.

SOME BASIC STRATIGRAPHIC TERMS AND CONCEPTS

Formations.—These units are the earliest recognized in the history of geology and are still the indispensable basis of all stratigraphic work in any given area, as well as of geologic mapping. The English formations were first named (mainly from the Jurassic) by William Smith between 1799 and 1815, and were based on a combination of lithologic and paleontologic characters (e.g. Fuller's Earth, Great Oolite, Forest Marble, Cornbrash, Kellaways Beds, Calcareous Grit, Coral Rag). Smith entitled his chief book defining his formations *Strata Identified by Organized Fossils.* In it he described both the lithic and paleontologic features of each formation, illustrating the characteristic assemblage of each on a plate colored to match the rock.

Schindewolf (1954, p. 26) asserts that the concept "formation" in English-language literature is completely different from that in the German.

This may be true for Paleozoic systems (of which I have no special knowledge) but it certainly is not true for the Mesozoic, in which the ancient formations in all European countries are precisely comparable to their English prototypes. For instance, the Upper Jurassic and Lower Cretaceous have been divided in northwest Germany for about a century into the following formations:

Hilston
Wealdenbildungen
Süsswasserkalke
Serpulit
Münder Mergel
Einbeckhäuser Plattenkalk
Gigas-Schichten
Kimeridge-Formation
Korallenoolith
Heersumer Schichten
Ornatenton

These are analogous in every way with the classic English formations and with those in use in France, Switzerland, Portugal, and other European countries. They cannot, of course, be correlated across the European frontiers without the interposition of a process of faunal analysis and the correlation of each with the abstract time-scale based on zones. Nevertheless each formation is almost as much a (crudely) paleontologic as lithologic unit.

Schindewolf is therefore right in objecting to Hedberg's classifying "formation" as a "litho-stratigraphic" unit, and further in objecting to the too clear-cut distinction between litho-stratigraphic and bio-stratigraphic concepts. These objections rest on a historical basis; but Hedberg is right in his placing of "formation," when the word is used in the purely trans-Atlantic, and in particular the United States, sense. The American "formation" has tended to become a pure rock term. To the European (including British) reader, the vast numbers of American formation names, which of themselves convey nothing, being simply arbitrary names for locally defined packets of anonymous rock, are a serious hindrance to comprehending many modern American papers.

Zones.—As everyone knows, neither d'Orbigny nor Oppel defined what he meant by zones, but most stratigraphers try to follow Oppel's usage. In both British and American literature a zone has most commonly been regarded as a tangible bed or band of rock, of any thickness from inches to hundreds of feet and any lithology or combination of lithologies, but always characterized by a certain assemblage of fossils, of which one is selected as index species.

Whatever may be the theoretical definition, in practice a zone can be recognized only *where* the characteristic fauna or index fossils lived and are preserved. Nearly all characteristic zonal fossils have a more or less restricted distribution. It is therefore true to say that "zones have a more or less limited horizontal extension," a statement which Schindewolf (1954, p. 32) takes from Jeletzky, 1951, and denies, but which is only a paraphrase of a statement

of mine (Arkell, 1946, p. 10) and is entirely justified by Oppel's own writings (as will be shown below by quotations).

Schindewolf reasserts (1954, p. 32) his extreme view (1950, p. 27) that the zone as conceived by Oppel was a purely chronological notion and that the term zone should be used only as a time term (= secule). He interprets Oppel's famous "ideales Profil, dessen Glieder gleichen Alters in den verschiedenen Gegenden immer wieder durch dieselben Arten charakterisirt werden" (1856, p. 3) to mean that Oppel "subtracts all qualitative and spatial characters of the strata, and what is left over after this subtraction, as the common element of the various rock members, is the time element"; and hence that "zone" is purely a time term.

Now I have been reading and using Oppel's classic works for thirty years and I emphatically reject this interpretation. The only characters that Oppel subtracts are the lithologic: "mit Hintansetzung der mineralogischen Beschaffenheit der Schichten" (disregarding the lithological nature of the beds) (Oppel, 1856, p. 3), and the "common element," on which Oppel everywhere insists and which is left after he subtracts the local lithological details, is *the characteristic fauna*. He says so himself in the famous sentence quoted ("durch *dieselben Arten* charakterisirt") but makes no mention of a time element except as a deduction from the characteristic fauna. Without the fauna a zone is nothing: a will-o'-the-wisp, without substance, unrecognizable. Its ascertainable existence, and therefore its usefulness for all practical purposes, is limited strictly by the area of occurrence of its index fauna or species. For other regions outside this area, separate zonal successions have to be built up, based on the faunas available; the synchronization of the different columns obtained for different faunal realms or provinces is the task of correlation.

In order to lay, I hope once and for all, this specter of an Oppelian zone without substance, and to show that it is a figment of Prof. Schindewolf's imagination, I append some typical quotations from Oppel's first and last works on zones. Scores of other passages could be chosen; these are taken at random and are perfectly representative.

From Oppel (1856-1858) p. 520; of the faunal list for the Athleta Zone: "Damit kommen noch weitere Arten vor, welche aber schon zum Theil in der darunterliegenden Zone des Amm. anceps auftreten." (With these occur also other species, which however already in part appear in the underlying zone of Am. anceps.)

"Als solche Arten, welche in der Zone des Amm. athleta beginnen, jedoch in die darüberliegenden Schichten hinaufgehen, bezeichne ich. . . . " (As species that begin in the zone of Am. athleta, but nevertheless go up into the overlying beds, I cite)

p. 521. "In Beziehung auf ihre organischen Einschlüsse lassen sich die schwäbischen Ornatenthone in 2 Zonen sondern, welche sich jedoch äusserlich gleichen. Die untere Hälfte, d.h. die Zone des Amm. anceps ist besonders zu Oberlenningen und Gammelshausen bei Boll deutlich aufgeschlossen. Man findet darin mit Ausnahme der Brachiopoden die vorhin erwähnten Leitmuscheln aus der Zone des Amm. athleta. Günstige Aufschlüsse bieten die Umgebungen von Ehningen," etc. (According to their contained organic remains, the Swabian Ornatum Clays can be divided into two zones, which nevertheless are superficially similar. The lower half, namely the zone of Am. anceps, is exposed especially clearly at Oberlenningen and Gammelshausen, near Boll. One finds in it, except for the brachiopods, the previously mentioned characteristic fossils of the zone of Am. anceps. It is only above this that the zone of Am. athleta follows on. Favorable exposures are offered by the neighborhoods of Ehningen, etc.)

From Oppel's last work, published ten years later, posthumously (1866):

p. 213. "Bisher wurde diese Zone [Transversarium Zone] als solche in Franken, Schwaben, dem Grossherzogthum Baden, dem Aargauer und Neuchâteler Jura bis in die

Départements Jura, Côte d'Or, Ain und Ardèche nachgewiesen und beschrieben. Für andere Gegenden war ihr Vorkommen durch das Auftreten von Spongien-Kalken oder einzelne bezeichnende Leitmuscheln gleichfalls angedeutet." (Hitherto this zone [Transversarium Zone] has been reported and described as such in Franconia, Swabia, the Grand-duchy of Baden, the Aargau and Neuchâtel Jura, and as far as the Departments of Jura, Côte d'Or, Ain, and Ardèche. For other districts its presence has been likewise indicated by the occurrence of sponge limestones or isolated characteristic fossils.)

p. 220. "Die Liste von 217 Arten, welche im Anhange aufgezählt werden, trägt nur im Allgemeinen dazu bei, ein Bild über das Vorkommen organischer Reste in der Zone des *Ammonites transversarius* zu geben." (The list of 217 species, enumerated in the appendix, contributes only in a general way to give a picture of the occurrence of fossils in the zone of *Am. transversarius.*) . . . . "auf alle diejenigen Bildungen angewendet, an welche die Zone eine an Cephalopoden, [etc] reiche Fauna enthält." (. . applied to all those formations in which the zone contains a fauna rich in cephalopods, etc.)

p. 226. "Die folgende Tabelle gestattet eine Uebersicht über die Anordnung der Einzelnen Facies in einigen Theilen des Verbreitungsgebiet der Zone des *Amm. transversarius.*" (The following table shows schematically the arrangement of the various facies in some parts of the area of distribution of the zone of *Am. transversarius.*)

Thus at these and countless other places throughout his books Oppel states unequivocally that his zones: (1) overlie or underlie other zones, (2) overlie or underlie beds, (3) contain fossils, (4) can be the lower or upper half of a Formation (e.g. Ornatum Clays), characterized by containing particular fossils although similar lithologically to the other half of the Formation, (5) can be exposed, in some districts more favorably than in others, (6) are reported and described in certain districts only, (7) can occur in various, typical or less typical facies, (8) possess "an area of distribution," i.e. are not ubiquitous.

The truth, therefore, is that a zone as conceived and employed by Oppel is not, and never was, a time term.

Stages.—For the purpose of constructing a single global time-scale, zones are unsuitable units because of their too-limited horizontal and vertical extent. It is at this juncture that abstraction enters. For correlation over long distances, where zones are horizontally too restricted and vertically too precise, a larger stratigraphic unit is required. It must correspond to groups of zones and be capable of universal extension by means of overlapping correlations, although based ultimately on a standard zonal succession at a type locality or in a type area. For these requirements the ideal unit, in the Mesozoic at least, is the stage.

Stages have been little used by German stratigraphers, being essentially of French origin. It is to be hoped that Schindewolf's (1954, p. 36) placing of a stage in his table of terms as the stratigraphical equivalent of the zone (used as a time term) will not be followed by German authors, for it makes nonsense of all previous responsible usage and deprives a very useful stratigraphic concept of all value whatever. If stages can find no place better than this in the Devonian or whatever system Schindewolf had in mind, this merely illustrates what has been pointed out before (Arkell, 1946, p. 2), that it may be impossible to standardize a single set of stratigraphic terms for all systems, because of the great variety of problems presented by different parts of the stratigraphic column, and of customs prevailing among the workers in different systems. Even within a single system it sometimes happens that the differentiation of faunal realms was so complete that at certain times not

even a stage comprising a whole packet of zones can be recognized all over the world.

As with other stratigraphic units, the multiplication of local stage names defeats the ends of stratigraphy. Local naming of the Mesozoic of Europe in the past has led to a stratigraphical Babel: there are nearly 130 "stage" names now available for the Jurassic alone. Of these eleven are useful and all that is necessary (Arkell, 1946, p. 4-5; 1956, p. 7). Each can be divided usefully into Lower, Middle, and Upper (or in some cases merely Lower and Upper), giving about 30 divisions. Each is based on a fixed combination of zones at the type locality of the stage. The lowest subdivision of the Oxfordian stage (for example) is the Lower Oxfordian; but if a definition of the Lower Oxfordian is demanded, reference has to be made to the type area, where it is defined as the zones of *Quenstedtoceras mariae* and *Cardioceras cordatum*.

Ages.—Hedberg (1954, p. 223) states the correct definition of "age": "the geologic-time unit corresponding to stage is age." It should, however, be pointed out that the term is also used in an altogether different sense. For Buckman and some of his British followers the term age denotes the time of dominance of a particular ammonite genus or family; thus Cardioceratan Age, the time of dominance of the family Cardioceratidae. Schindewolf (1954, p. 34) considers adopting these "handy terms" (with suffix changed to -ium) in German literature, and Neaverson (1955) still uses them as the sole stratigraphical unit for a world review of stratigraphy (in the form "Cardioceratan Fauna").

I wish to go on record with the considered opinion that these Buckmanian age terms serve no useful purpose; that they have all the limitations of, and are for the most part merely paraphrases of, Oppel's zones; and that they cannot take the place of stages for world correlation because, like zones, they are strictly limited by the area of occurrence of the name fauna. The old classical species of the mid-nineteenth century, which were selected by Oppel as index fossils for his zones, have for the most part now become genera, with the result that the dominant species of Oppel's day is much the same as the dominant genus of today. It results that Buckman's ammonite "ages" (Neaverson's "faunas") are more or less reflections of Oppel's zones. They suffer from all the limitations of zones and cannot be used for world correlation or world dating. No one can recognize the Cardioceratan Age outside the northern hemisphere, for outside it no *Cardioceras* is yet known to exist. It is, however, possible to recognize the Oxfordian or Callovian stages, for those are abstractions, not dependent on occurrence of any particular index species or index genera, but recognized by the general grade of evolution of the ammonite fauna as a whole and connected with the European type area by a chain of overlapping correlations carried link by link round the world (Arkell, 1956, p. 11-12).

STRATIGRAPHIC PROCEDURE, PAST AND PRESENT

To illustrate the simplicity of the processes employed by the stratigrapher, the following tables represent the steps in the first description and stratigraphic treatment of a part of the Middle and Upper Jurassic of England by William

Smith, Oppel, and d'Orbigny (with some elaborations or refinements by their successors), and the similar treatment, about a hundred years later, of a previously unknown tract in central Arabia by Messrs. Steineke, Bramkamp, and Arkell (in that order). It will be seen that the processes are essentially the same and that swarms of complicated technical terms are unnecessary.

In column A in both tables, the formation names denote convenient rock units defined on lithic and paleontologic characters. The steps by which William Smith proceeded were repeated almost 140 years later in Arabia by Steineke and Bramkamp. This is the indispensable preliminary work, called rather disparagingly by Schindewolf "Prostratigraphy," in his view hardly stratigraphy at all, but in reality a sine qua non.

TABLE 1

Parts of the Middle and Upper Jurassic of Southern England

A. Formations (mainly Wm. Smith, 1799-1815)	B. Zones (mainly d'Orbigny, 1850-1852,* and Oppel, 1856-1863)	C. Stages (d'Orbigny, 1850)**
Upper Oxford Clay	cordatum mariae	Lower Oxfordian
Lower and Middle Oxford Clay	athleta coronatum jason	Callovian
Kellaways Beds	calloviense	
Great Oolite Series: Upper Cornbrash	macrocephalus	
Great Oolite Series: Lower Cornbrash	discus	Bathonian
Great Oolite Series: Forest Marble		
Great Oolite Series: Great Oolite and Fuller's Earth Rock	subcontractus	
Great Oolite Series: Lower Fuller's Earth	zigzag	
Upper Inferior Oolite	parkinsoni garantiana subfurcatum	Upper Bajocian

* The pertinent pages were published in 1850-1852.

** The pertinent pages were published in 1850.

Column B shows that the zones in the two areas have not a single name in common. In table 1, column B represents a carefully selected list chosen (mainly by Oppel) after analysis of the total faunas, with the object of spotlighting the most characteristic or stratigraphically most useful ammonite species. In table 2 the list results from precisely the same process carried out by Bramkamp and Arkell. The fact that there is no species in common between the two lists provides the final answer to the question whether or not zones are limited in horizontal extent. (The distance is only one-sixth of the way round the Earth.) Assertions that zones are of unlimited extent lack any basis in reality and are a sad object lesson in the dangers of arguing from the general to the particular.

TABLE 2

Parts of the Middle and Upper Jurassic of Central Arabia
(after Arkell, Bramkamp and Steineke, 1952)

A. Formations	B. Zones	C. Stages (world correlation)
Hanifa		?Oxfordian
Tuwaiq Mt. Limestone	Erymnoceras spp.	Middle Callovian
Upper Dhruma	Gryphaea costellata	?Upper Bathonian
Middle Dhruma	Dhrumaites sp. Micromphalites spp. Tulites spp.	Middle (and Upper?) Bathonian
	Thambites spp.	?Lower Bathonian
Lower Dhruma with Dhibi Limestone at top and other beds below	Ermoceras spp.	early Upper Bajocian
	Dorsetensia arabica	Middle Bajocian

Not until column C do the two tables begin to show any common factors. Between columns B and C in table 2 has been interposed a further process of analysis of the faunas in column B and comparison of them with other faunas in all parts of the world, to establish as nearly as possible to which elements in the standard European succession they are most likely to correspond. This enables a rough translation to be made into stages, and only then are the Arabian faunas and formations "dated." In table 2 the process of obtaining column C from column B bristles with difficulties: *Gryphaea costellata* is in no country unequivocally associated with datable ammonites; *Dhrumaites* and *Thambites* are new genera, not known outside Arabia; *Ermoceras* was previously known only in Sinai in beds of doubtful age, but it has since been found in Algeria in an asemblage certainly of the Subfurcatum Zone (early Upper Bajocian). The three most useful zones were those of *Erymnoceras* (presumably Coronatum Zone of Europe), *Tulites* (presumably Subcontractus Zone of Europe), and *Dorsetensia* (presumably Middle Bajocian as in Europe). But the only *Micromphalites* known in Europe (in one place in France and one place in England) occur below the horizon of *Tulites*, whereas in Arabia they are found above the *Tulites* horizon.

This example proves the thesis maintained here, that stratigraphic processes are essentially the same now as in the days of Smith and Oppel who had only a few basic terms to work with.

CORRELATION

All English-speaking stratigraphers know very well what they mean by correlation. It is at the center of our activities: our highest function, the ultimate goal of most of our plodding work. It is only necessary here, therefore, to defend the word itself from a surprising attack, before anyone takes that attack seriously and introduces a new substitute word. Prof. Schindewolf is rash indeed in coming out with a philological broadside against a word in

a language not his own and clearly he is mistaken in asserting (1950, p. 15; 1954, p. 29) that "correlation" "in the English language is correspondence between objects and concepts which stand in opposite relation to one another. . . . Correlation therefore means something that is, but that one cannot do. The taking over of this concept in application to the equating of strata is therefore impossible; the geologist can compare, arrange, classify, co-ordinate and equate, but not 'correlate'."

Perhaps Prof. Schindewolf has overlooked the flexibility of language and its capacity for growth. He has attacked a word adopted by English-speaking geologists at least 106 years ago and long since sanctioned and incoporated (as a verb) into the vocabulary with a meaning that is essentially uniformly understood. In the *Oxford English Dictionary*[1] stands "Correlate, verb 2. To place in or bring into correlation; to establish or indicate the proper relation between (*spec.* geological formations, etc.)." The first example given is from Murchison's "Siluria," vii, p. 134, 1849: "Mr. Symonds was enabled to correlate these beds with their equivalents near Ludlow." Murchison (1849) is also quoted under the noun "correlation," which goes back to at least 1561, with essentially the geological meaning.

STRATIGRAPHIC NOMENCLATURE AND TYPOGRAPHY

One of the matters which it is to be hoped the Stratigraphic Commission of the International Geological Congress will clear up is the question of how stratigraphic names should be written and printed. At present the matter is more chaotic than it used to be because of the recent introduction by some publishing societies of house rules which were ill thought out and unfortunately compel authors to appear as sponsors for outlandish and non-traditional forms which they would repudiate if they could.

The trouble arises when a stratum is named after a fossil. If the fossil name is adopted from the vernacular, Roman type is always employed (Coral Rag, Ammonite Marble), and in many familiar cases no departure has been customary when the name is the Latin name of a fossil, either generic or specific (for example, Clypeus Grit, Boueti Bed, Bucklandi Limestone, Acuminata Beds, Humphriesianus-Schichten, Arietenbank, Caillasse à Anabacia, etc). A glance through stratigraphic literature of the past 150 years, however, shows that there has been no consistency in the use of italics or Roman for such names. The only point that, until recently, was invariably consistent was the use of capital initial letters for stratal names, irrespective of whether the fossil name adopted was specific or generic. (See examples just cited.)

Recently, there seems to have been a tendency in some quarters to confuse Zoological Nomenclature with Stratigraphical Nomenclature and to mishandle purely stratigraphical names in a mistaken attempt to make them conform to zoological nomenclature. Thus certain societies have enforced as a house rule in publishing their journals (and others have followed merely for the sake of uniformity) the printing of such names not only in italics but also

[1] 13 volumes, folio (Oxford, 1933): "Unrivalled in completeness and unapproachable in authority"—(Prime Minister Baldwin); "There is no dictionary in the world to compare with it"—(Times Literary Supplement); "The greatest single enterprise of scholarship the world has known"—(Spectator).

with small initial letter. There result such forms, now commonly seen, as "*acuminata* Beds," "*bucklandi* Beds," "*subnodosocostatum* Zone," which look grotesque and are non-traditional and, I submit, incorrect. Before the process goes any farther it is to be hoped that stratigraphers will reconsider the matter on its merits and make a stand for tradition before and in case we have uniformity of error.

When J. Sowerby honored Dean William Buckland by naming an ammonite after him, the patronymic Buckland had to undergo a transformation in three particulars to conform to the rules (or conventions, as they then were) of zoological nomenclature: it lost its initial capital, went into italics, and acquired a genitival suffix. Thus Buckland the patronymic, on becoming a part of zoological nomenclature as the name of a species of ammonite, became *bucklandi*.

In the same way, when a fossil is chosen for the name of a stratigraphic formation, it ceases to be the name of a fossil and becomes part of the name of a formation or bed, and should undergo another metamorphosis accordingly, to conform to the conventions of stratigraphic nomenclature. Thus the limestone with *Ammonites bucklandi* Sowerby becomes the Bucklandi Limestone or Bucklandi Zone.

As a postscript note it may be mentioned that French practice has not yet adopted the convention of using a small initial letter for species based on a patronymic even in zoological nomenclature: the French still always write *Ammonites Bucklandi* or *Arietites Bucklandi*. Furthermore, it is a house rule of the Paris Academy of Sciences not to allow the placing in parentheses of the author of a specific name when the genus has been changed: they insist on the form *Arietites Bucklandi* Sowerby where all the rest of the world would print *Arietites bucklandi* (Sowerby) or *Arietites bucklandi* Sowerby sp. Even the conventions of zoological nomenclature, therefore, are not yet universally standardized, and house rules of even the most august of societies may be in error.

CHANGING NAMES OF STRATIGRAPHIC UNITS FOR POLITICAL OR OTHER INADEQUATE REASONS

Another matter for which the International Congress might usefully consider issuing a rule is the irresponsible changing of the names of stratigraphic units. Zoological nomenclature has an excellent rule that once a species or genus is named, if the name is valid technically, it cannot be changed on grounds of inappropriateness or for any other reason. Occasionally this rule gives rise to some curiosities, as with the Canadian Callovian ammonite genus *Seymourites*, named after Seymour Island in Antarctica, where it does not occur but was mistakenly believed to occur in the Upper Cretaceous; but the rule does make for stability and is on balance of great benefit to zoological (and paleontological) nomenclature. It is here suggested that a similar rule is needed for stratigraphic nomenclature. At present names of well known formations, groups, or series are wide open to be changed after every revolution in any country. It is bad enough when pre-revolution literature becomes

unintelligible because all the place names change, but at least the stratigraphic units based on the old names might be protected from change.

One of the world's classics in Mesozoic paleontology is Waagen's (1873-1875) monograph on the Jurassic ammonites of Kutch, which was based on a stratigraphy adopted by the Geological Survey of India. The four formations or "groups" into which the Kutch Jurassic was divided—Patcham, Chari, Katrol, Umia—have been world currency for 80-90 years. It now appears that detailed mapping has revealed that the village of Chari is "several kilometres" off the Chari outcrop, and that "there are good exposures of Chari Beds at Habo Hill," and so the Chari Group is renamed "Habo Series" (Agrawal, 1955). This is precisely the situation against which fossil names are now protected by the rule of the International Commission against renaming on account of inappropriateness.

GENERAL COMMENTS

(a) Stratigraphical nomenclature is distinct from zoological nomenclature, and it is time that the International Geological Congress recognized and proclaimed the fact. Stratigraphical nomenclature should have its own rules of typography, appropriate to its own needs and traditions, and independent of zoological conventions such as italics and small initial letters for names taken over from paleontology. It would, however, benefit by the proclamation by the Congress of some rules analogous with some of those governing zoological nomenclature: especially the rule protecting current names from change on insufficient grounds.

(b) Hedberg's breakdown of stratigraphic procedural terms into those appropriate to litho-stratigraphy, bio-stratigraphy, and chrono-stratigraphy is not entirely consonant with the facts, and most of Schindewolf's criticisms of this analysis are valid. On the other hand some of Schindewolf's suggested emendations are even worse, and his pronouncements as to the meaning and characteristics of formations, zones, stages, and correlation are baseless.

(c) In stratigraphic procedure, it is not what terms an author uses that matters, but whether he knows what he is talking about. If he does, he will be unambiguous however few and simple the terms he employs, but if he does not, the more technical terms he uses, and the more technical those terms are, the greater will be the danger of confusion and misapprehension. The mere existence of an excessive number of technical terms is a danger to clarity because of the attraction they have for the tyro. Accordingly I agree wholeheartedly with Schindewolf (1954, p. 31) in his plea for the standardization and employment of only the minimum necessary number of stratigraphic terms. Some terms, such as group and series, have been used in the past with many different meanings and will remain more useful if not formalized by strict definitions. It would be a mistake to leave no unrestricted words of this kind in the common pool of language.

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THE LOWER LIMIT OF STABILITY OF AKERMANITE ($Ca_2MgSi_2O_7$)*

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ABSTRACT. Experiments have been carried out to determine the P-T curve for the reaction, Wollastonite + Monticellite $\rightleftarrows$ Akermanite. The P-T curve lies above 700 and below 750°C between 30,000 and 60,000 lbs./in.^{2}.

This P-T curve adds another line to the petrogenetic grid and, being nearly parallel to the pressure axis it must cross or intersect other P-T curves for reactions involving volatile phases.

Wollastonite-monticellite assemblages, e.g., at Crestmore, are to be expected in rocks of appropriate composition if the ultimate crystallization were at a temperature below the stability field of akermanite and in the absence of excess CO_2.

INTRODUCTION

Bowen, Schairer and Posjnak (1933) found that when a glass of akermanite composition was crystallized at 1327°C. akermanite crystals alone developed. When, however, the same glass was crystallized at 1050°C., the akermanite which formed was accompanied by other crystallites identified as Ca_2SiO_4. In view of this they suggested that akermanite becomes unstable with falling temperature. Later, Osborn and Schairer (1941) found that above 1325°C. akermanite crystals grown at 1400°C. remained homogeneous but that below this temperature small euhedral crystals developed in the akermanite, and some of these had the refractive index and general appearance of diopside.

The present study shows that akermanite breaks down to wollastonite + monticellite at temperatures well below 1000°C.

EXPERIMENTAL PROCEDURE AND RESULTS

The experimental work was carried out in cold seal pressure vessels (Tuttle, 1949) using the apparatus described in connection with studies in the CaO—MgO—CO_2 system (Harker and Tuttle, 1955). In the present study, however, water was used to apply a hydrostatic pressure instead of CO_2, as it was required that the fluid phase would not react either with akermanite or with its breakdown products.

By making runs at different pressures and temperatures it has been possible to determine the P-T curve for the univariant reaction:

$$CaSiO_3 + CaMgSiO_4 \rightleftarrows Ca_2MgSi_2O_7$$
(wollastonite) (monticellite) (akermanite)

No hydrates were formed, so the only function of the water was to transmit the hydrostatic pressure to the solid phases and to act as a flux; water did not enter into the reaction as an active component in any other way.

The starting materials used in the runs were: (A) A fine-grained intimate mixture (Mix IV in table 1) of CaO, MgO and SiO_2 in the molecular proportions 2:1:2 corresponding to the akermanite composition. The CaO was prepared by firing a weighed amount of $CaCO_3$. The $CaCO_3$ and the MgO were the analytical reagents of the Fisher Scientific Company. Silica

* Contribution no. 55-8 from the College of Mineral Industries, The Pennsylvania State University.

glass was used as the source of SiO_2. (B) A fine-grained intimate mixture of akermanite, wollastonite, and monticellite formed by the partial conversion of wollastonite + monticellite (in akermanite proportions) to akermanite with rising temperature. (This is Mix III in table 1). (C) Synthetic akermanite formed by the rapid crystallization of a liquid of akermanite composition.

The products of the runs were identified by their optical properties and their X-ray powder diffraction patterns.

During runs made on the high temperature side of the P-T curve akermanite developed from the two mixtures while the synthetic akermanite remained as the stable phase. On the low temperature side of the curve wollastonite + monticellite developed from the mixtures, and the synthetic akermanite broke down to wollastonite + monticellite. At 610°C. and 41,000 lbs./in.^{2} akermanite broke down completely to wollastonite and monticellite in euhedral acicular and subequant crystals respectively, in ten days. Likewise, by reacting the mixtures for 15 hrs. at 798°C. and 58,000 lbs./in.^{2}, pure akermanite could be prepared hydrothermally.

At pressures and temperatures closer to the P-T curve reaction rates are very much slowed down. However, it was not necessary for the reaction represented by the P-T curve to go to completion in order to ascertain on which side of the curve a given pressure-temperature condition belonged. It was sufficient to be able to detect in which direction the reaction was going. Thus the formation of a small amount of akermanite from the oxide mix or the development of a small amount of wollastonite + monticellite from the synthetic akermanite indicated conditions on the high or low temperature sides of the P-T curve respectively.

It was considered advisable, however, to make certain that in such runs the new phases were actually steadily growing and that they were not present solely as a result of a sporadic metastable development due, say, to the variable P-T conditions to which the sample was necessarily subjected during the short period of the initial heating at the beginning of the run. Some of the runs, therefore, were repeated under a given set of P-T conditions for different lengths of time. In this way the continued growth of the stable assemblages could be observed. Mixture III was particularly useful in this respect, as it was most easy to see, by optical means and especially by the changing intensities of the X-ray powder diffraction lines, whether akermanite was increasing or decreasing in amount with respect to wollastonite and monticellite.

The successful runs made to determine the P-T curve are listed in table 1 from which figure 1 has been drawn.

DISCUSSION OF RESULTS

The breakdown of akermanite to wollastonite + monticellite is very different from the breakdown to calcite + diopside, which takes place at much higher temperatures in the presence of excess CO_2—see figure 2, curve I (Harker and Tuttle, unpublished data). In the breakdown to wollastonite + monticellite, akermanite becomes unstable by virtue of its own internal structure and not through any tendency to react with other phases which

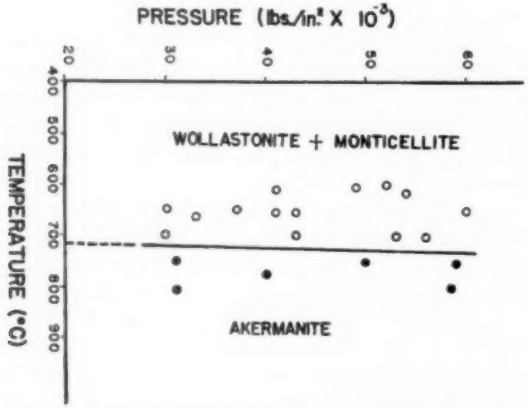

Fig. 1. P-T curve for the univariant reaction: wollastonite + monticellite ⇄ akermanite. o = runs made on the low temperature side of the P-T curve where wollastonite + monticellite is the stable assemblage. • = runs made on the high temperature side of the P-T curve where akermanite is stable.

may be present (as for example, H_2O or CO_2). On crossing the P-T curve for this reaction, in which solid phases only are involved, the volume change (ΔV in the equation below) will almost certainly be both small and nearly constant in comparison with what it would be were a volatile involved. (The volume change, using density values determined at room temperature and atmospheric pressure, is in fact such that high pressures might be expected to favor the wollastonite-monticellite assemblage.) It is anticipated, therefore, from the Clausius Clapeyron equation, $\frac{dp}{dt} = \frac{\Delta H}{\Delta V}$, that the P-T curve will be steep and have a nearly constant slope. The curve itself will be essentially a straight line intersecting the temperature axis on the P-T diagram at a large angle, unlike the P-T curves for dehydration and decarbonation reactions which, at the lower pressures and where the fluid phase is either above the critical point or in the gaseous state, become more and more nearly parallel to the temperature axis as $\frac{dp}{dt}$ decreases with falling pressure and temperature.

Assuming that the P-T curve is straight and has a positive slope as is suggested by ΔV, $\frac{dp}{dt}$ can vary between ∞ and 600 lbs./in.²/°C. Because ΔH is infinite if $\frac{dp}{dt}$ is infinite, it is not possible to estimate an upper limit for the value of ΔH. As, however, $\frac{dp}{dt}$ cannot be less than 600 lbs./in.²/°C. if the P-T curve is to be between the experimentally determined limits, then ΔH cannot be less than about 37K.cals/mole at 750°C. or 35 K.cals/mole at 700°C.

The P-T curve for the breakdown of akermanite to wollastonite + monticellite will cross or intersect several other P-T curves which have slopes different from that of the akermanite breakdown curve. Some of the more important of these are illustrated in figure 2 representing the reactions: (a) clinochlore $\rightleftarrows$ forsterite + cordierite + spinel + H_2O (Yoder, 1952)—curve VI; (b) quartz + calcite $\rightleftarrows$ wollastonite + CO_2 (Harker and Tuttle, 1956)—curve V; (c) magnesite $\rightleftarrows$ periclase + CO_2—curve IV; and (d) dolomite $\rightleftarrows$ calcite + periclase + CO_2 (Harker and Tuttle, 1955)—curve III.

These curves constitute a contribution to the petrogenetic grid envisaged by Bowen (1940, p. 273) which, when accurately established on the basis of a sufficient number of P-T curves, may well provide the most satisfactory foundation for a reclassification of all metamorphic rocks on the Facies principle.

The P-T curves for dehydration and decarbonation reactions as determined in the laboratory are only strictly applicable when the partial pressures of water and carbon dioxide are known. Owing to the ability of volatiles to escape through fissures, it can only be deduced that the stable existence of a certain phase assemblage indicates that the P-T conditions have never attained the high temperature side of the P-T curve. The occurrence of wollastonite in a rock, for example, does not necessarily mean that the rock has crystallized under general P-T conditions on the high temperature side of curve V in figure 2, for this curve applies only to systems with excess CO_2. On the other hand, with P-T curves of purely solid phase reactions, it is unnecessary to consider the partial pressures of fluid phases present. This is a great advantage; it means that if akermanite occurs stably, no matter with what other phases, then P-T conditions belonging to the high temperature side of the curve *must* have been attained.

Although rocks of the right composition to bear akermanite, or wollastonite + monticellite, are unlikely to contain clinochlore, dolomite, or magnesite, nevertheless, it is possible that in a small region, representing a single set of P-T conditions, the composition of the rocks may vary sufficiently to make all the above, and other reactions possible. The usefulness of the grid would then be evident. For example, the stable occurrence of clinochlore in a rock in which akermanite-bearing strata occur would indicate that the rock crystallized under P-T conditions in the divariant region between curves VI and VII but on the high pressure and high temperature side of the cross-over. On the other hand, if clinochlore-bearing rocks were closely associated with wollastonite-monticellite-bearing rocks, then this would indicate that the P-T conditions of crystallization belonged to the divariant region between curves VI and VII but below the cross-over.

It is to be expected, as already stated above, that at least at the lower pressures the P-T curves for reactions involving solid phases only will be more nearly vertical than P-T curves representing reactions in which a fluid phase is involved. It is clear, however, that P-T curves for reactions involving fluid phases may cross one another. Curve VI, for example, crosses curves V and IV, and it is strongly suspected that curves I and II (Harker and Tuttle,

TABLE 1

$CaSiO_3 + CaMgSiO_4 \rightleftarrows Ca_2MgSi_2O_7$

Run	Initial materials	Pressure lbs./in.^{2}	Temp. °C.	Time hrs.	Products
1011	Mix III	60,000	650	96	Ak ——> Wo + Mo
1012	Ak	"	"	"	Ak + Wo + Mo
1168	Mix IV	59,000	750	5	Wo + Mo + Ak
1160	"	"	"	16	Ak + Wo + Mo
1169	Mix III	"	"	5	Wo + Mo ——> Ak
1161	"	"	"	16	Wo + Mo ——> Ak
1170	Ak	"	"	5	Ak
1162	"	"	"	16	Ak
1154	Mix IV	58,500	798	15	Ak
1155	Mix III	"	"	"	Ak
1156	Ak	"	"	"	Ak
1050	Mix III	56,000	700	48	Ak ——> Wo + Mo
1051	Ak	"	"	"	Ak + Wo + Mo
1086	Mix III	54,000	615	240	Wo + Mo
1087	Ak	"	"	"	Wo + Mo + Ak
1033	Mix III	53,000	700	168	Ak ——> Wo + Mo
1034	Ak	"	"	"	Ak + Wo + Mo
1052	Mix III	52,000	600	40	Ak ——> Wo + Mo
1053	Ak	"	"	"	Ak + Wo + Mo
1094	Mix III	49,000	605	120	Wo + Mo
1095	Ak	"	"	"	Ak + Wo + Mo
986	Mix III	43,000	655	192	Ak ——> Wo + Mo
987	Wo + Mo	"	"	"	Wo + Mo
989	Ak	"	"	"	Ak + Wo + Mo
969	Mix III	"	700	168	Ak ——> Wo + Mo
970	Wo + Mo	"	"	"	Wo + Mo
972	Ak	"	"	"	Ak + Wo + Mo
1102	Ak	"	"	2	Ak (+ Wo + Mo)
1101	Mix III	"	"	"	Ak ——> Wo + Mo
925	Mix III	41,000	610	240	Wo + Mo
926	Ak	"	"	"	Wo + Mo
1060	Mix III	"	655	"	Ak ——> Wo + Mo
1061	Ak	"	"	"	Ak + Wo + Mo
1015	Mix III	40,000	775	216	Ak
1016	Ak	"	"	"	Ak
891	Mix III	37,000	650	72	Ak ——> Wo + Mo
892	Ak	"	"	"	Ak + Wo + Mo
953	Mix III	33,000	665	120	Ak ——> Wo + Mo
954	Ak	"	"	"	Ak + Wo + Mo
1134	Mix IV	30,000	650	168	Wo + Mo
1135	Mix III	"	"	"	Ak ——> Wo + Mo
1136	Ak	"	"	"	Ak + Wo + Mo
1137	Mix IV	"	700	24	Wo + Mo (+ Ak)
1157	"	"	"	192	Wo + Mo
1138	Mix III	"	"	24	Ak ——> Wo + Mo
1158	"	"	"	192	Ak ——> Wo + Mo

TABLE 1 (Cont.)

$CaSiO_3 + CaMgSiO_4 \rightleftarrows Ca_2MgSi_2O_7$

Run	Initial materials	Pressure lbs./in.^{2}	Temp. °C.	Time hrs.	Products
1139	Ak	"	"	24	Ak
1159	"	"	"	192	Ak + Wo + Mo
1116	Mix IV	31,000	750	24	Ak + Wo + Mo
1122	"	"	"	"	Ak
1117	Mix III	"	"	"	Wo + Mo ——> Ak
1123	"	"	"	"	Ak
1118	Ak	"	"	"	Ak
1124	"	"	"	"	Ak
1111	Mix IV	"	805	1-1/3	Wo + Mo
1113	"	"	"	14	Ak + Wo + Mo
1106	"	"	"	44	Ak (+ Wo + Mo)
1114	Mix III	"	"	14	Ak
1107	"	"	"	44	Ak
1112	Ak	"	"	"	Ak
1115	"	"	"	"	Ak
1108	"	"	"	"	Ak

Legend: Mix III = Fine-grained intimate mixtures of Ak, Wo and Mo.
Mix IV = Fine-grained intimate mixture of CaO, MgO and SiO_2 (glass).
Ak = Akermanite, Wo = Wollastonite, Mo = Monticellite.
Products occurring only in trace amounts are shown in parentheses.

unpublished data) may intersect each other at pressures and temperatures somewhat higher than these for which data have so far been obtained.

Just as the clinochlore P-T curve is a dehydration curve involving excess water, curves I to V are decarbonation curves and only apply to systems containing excess CO_2. The reaction wollastonite + monticellite $\rightleftarrows$ akermanite could not take place in the same system as these decarbonation reactions because in the presence of excess CO_2 akermanite and monticellite become unstable, reacting to form calcite + diopside and calcite + diopside +forsterite respectively, at much higher temperatures—see curves I and II. Also, above at least 15,000 lbs./in.^{2} wollastonite becomes unstable in the *presence* of excess CO_2, reacting to form calcite + quartz (see curve V) at higher temperatures than those at which akermanite breaks down to wollastonite + monticellite in the *absence* of CO_2.

THE WOLLASTONITE-MONTICELLITE INTERGROWTHS AT CRESTMORE

The breakdown of akermanite as described in this paper may explain the occurrence of the wollastonite-monticellite assemblages in some of the contact-altered rocks at Crestmore, California (Dr. C. Wayne Burnham, personal communication). These rocks consist largely of calcium-magnesium-carbonate rocks which have some siliceous and aluminous impurities and which have suffered some metasomatism in addition to the thermal metamorphism caused by the intrusion of the quartz-bearing diorites and mon-

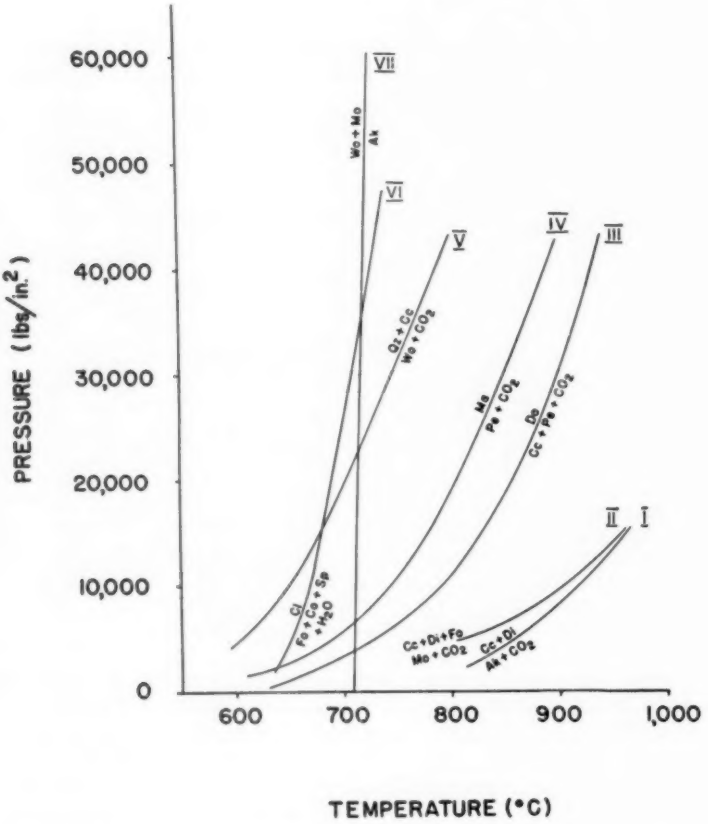

Fig. 2. P-T curve for the reactions:

I Calcite + Diopside $\rightleftarrows$ Akermanite + CO_2
II Calcite + Diopside + Forsterite $\rightleftarrows$ Monticellite + CO_2
III Dolomite $\rightleftarrows$ Calcite + Periclase + CO_2
IV Magnesite $\rightleftarrows$ Periclase + CO_2
V Quartz + Calcite $\rightleftarrows$ Wollastonite + CO_2
VI Clinochlore $\rightleftarrows$ Forsterite + Cordierite + Spinel + H_2O
VII Wollastonite + Monticellite $\rightleftarrows$ Akermanite

The location of Curve III below 3,000 lbs./in.² has been taken from Graf and Goldsmith (1955).

zonites. As a result, a great variety of minerals and mineral assemblages have been produced.

The authors became interested in the wollastonite-monticellite assemblages because at no stage in Bowen's decarbonation series are wollastonite and monticellite found together in stable equilibrium. That these two phases were in equilibrium with each other at some stage in the Crestmore rocks is strongly

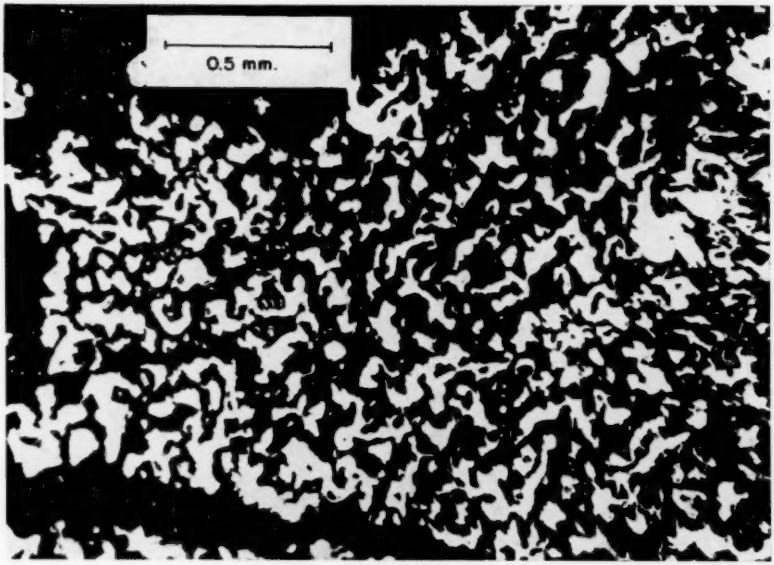

Fig. 3. Intimate intergrowth of wollastonite (in extinction) and monticellite (illuminated). Crossed nicols. Rock specimen kindly provided by Dr. C. Wayne Burnham.

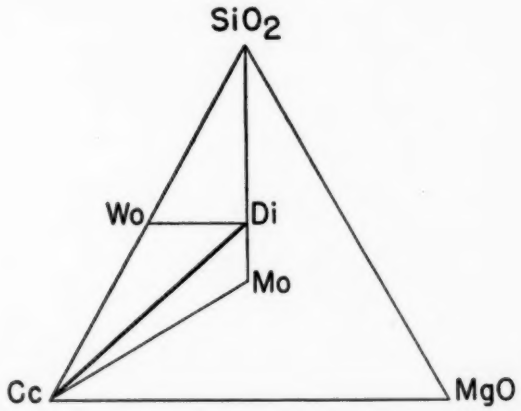

Fig. 4. Diagram illustrating the separation of the wollastonite composition from the monticellite composition by the Calcite-Diopside-CO_2 plane (with CO_2 at the eye of the observer) in the excess CO_2 part of the CaO—MgO—SiO_2—CO_2 system.

Wo = Wollastonite
Di = Diopside
Mo = Monticellite

suggested by the fact that they occur in an intimate intergrowth (fig. 3). A modal analysis made on enlarged photographs of four such intergrowths gave the following results:

Wollastonite	48.85	53.00	50.91	57.24	% volume
Monticellite	51.15	47.00	49.09	42.76	% volume

yielding an average of 52.5% by volume of wollastonite and 47.5% by volume of monticellite.

In the normal course of the decarbonation of siliceous dolomitic limestones in the presence of *excess* CO_2, as deduced by Bowen from observations of natural rocks in the system CaO—MgO—SiO_2—CO_2, assemblages including wollastonite + CO_2 are cut off from those including monticellite + CO_2 by the calcite-diopside-CO_2 plane (fig. 4). The stability of a calcite-diopside-CO_2 assemblage is well established at temperatures above and below those at which wollastonite and monticellite normally develop during decarbonation.

Even in the CO_2-*deficient* region of the CaO—MgO—SiO_2—CO_2 system, i.e., in the region below the calcite-diopside-wollastonite and the calcite-diopside-monticellite planes, the wollastonite-monticellite join is interrupted by

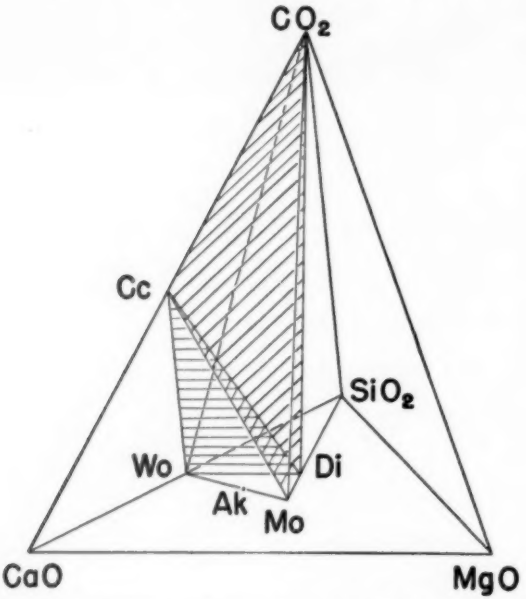

Fig. 5. The CaO-MgO-SiO_2 tetrahedron showing the Calcite-Diopside-CO_2 plane and the CO_2-deficient region below the Calcite-Diopside-Wollastonite and the Calcite-Wollastonite-Monticellite planes (shaded).

Cc = Calcite
Wo = Wollastonite
Ak = Akermanite
Di = Diopside
Mo = Monticellite

akermanite (fig. 5). However, now that the lower limit of stability of akermanite has been proved, there is no reason why wollastonite and monticellite should not occur in equilibrium together in CO_2-deficient systems under P-T conditions on the low temperature side of the akermanite breakdown curve.

An intergrowth formed solely by the breakdown of akermanite should consist of 45% wollastonite and 55% monticellite by volume. As mentioned above, the volume ratio averages 52.5: 47.5 instead of 45:55 in the Crestmore intergrowths. This discrepancy could be explained by the original presence of a little excess wollastonite or by impurity of the natural phases.

Clearly an important aspect to consider with reference to the application of the results to natural rocks is the effect of solid solution on the breakdown. Of the components most likely to enter into the akermanite the most important is gehlenite. No detailed work has yet been carried out on the effect of the gehlenite content on the lower limit of stability of melilite, but it has been suggested that the structure is stabilized by the addition of gehlenite and that the melilites become unstable at progressively lower temperatures as the gehlenite content is increased (Osborn and Schairer, 1941, p. 718).

Although in fact the wollastonite-monticellite intergrowths at Crestmore gave rise to the present investigation, it is possible that they have developed in some way other than by the breakdown of a melilite. They may have formed, for example, by the direct recrystallization of part of the rock which had a composition close to melilite, or they could represent an intermediate stage in the replacement of wollastonite by monticellite or vice versa. Crestmore has been noted for some time for intergrowths of different kinds (Dunham, 1933) which may have formed in a variety of ways. On the other hand, it should perhaps be pointed out that the co-existence of a melilite of say, composition (Ak_x,Geh_{100-x}) in equilibrium with wollastonite-monticellite intergrowths does *not* provide any evidence against the formation of the intergrowths by the breakdown of the akermanitic component of a melilite of composition (Ak_y, Geh_{100-y}), where $y > x$. The addition of gehlenite in solid solution must render the breakdown no longer univariant as indicated by the Gibbs Phase Rule. It is, in fact, to be expected that an original melilite (Ak_y, Geh_{100-y}) will, if it reaches its lower limit of stability, break down to a second melilite (Ak_x, Geh_{100-x}) + wollastonite + monticellite.

However, no matter what process caused the development of the wollastonite-monticellite intergrowths, if the two phases were in equilibrium, they must have formed on the low temperature side of the P-T curve for the breakdown of that particular melilite composition.

It will be possible to discuss more fully the usefulness of the P-T curve for the lower limit of stability of akermanite when the effect of solid solution of other components is known.

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ON THE EMPLACEMENT OF GRANITES

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ABSTRACT. This article is actuated by the recent paper of Walton (1955) in this Journal. The discussion of the emplacement and origin of granites should not be generalized to consider all rocks of the granite-granodiorite family, but a distinction should first be made between different members of this family. Particularly the role of potash feldspar, and especially the kind, must be considered; the conditions of formation of microcline, the prevalent potash feldspar in Precambrian granites, are as yet obscure but definitely limit the possibilities of the origin of potassic rocks.

The mode of occurrence of potash feldspar is plainly dissimilar in different granites. In the synkinematic granites it is definitely younger than other constituents; in the late-kinematic granites, on the contrary, the age relations between microcline and other constituents seem to be mostly indefinite.

Only the emplacement of late-kinematic (and post-kinematic) granites is considered to involve actual problems of the emplacement of granites. For most synkinematic "granites," the metasomatic origin is obvious, and they are frequently poor in potassium; their "emplacement" is hardly a problem. The synkinematic granites with much potassium are generally porphyroblastic, and their microcline has been mainly concentrated into the insets. Such granites form a separate problem.

INTRODUCTION

Two papers have recently been published in this Journal discussing the problems of the emplacement of granites. In the earlier, Perrin (1954) represented the extreme transformist point of view; in the later, Walton (1955) made certain critical comments regarding Perrin's paper and outlined a point of view obviously intermediate between the transformists and the magmatists.

Walton's paper gives a rather pessimistic picture of our present understanding of granites, and the reader gets the impression that the author wishes particularly to point out that the large majority of existing suggestions and ideas regarding the granite problem are based more or less either on insufficient actual knowledge, or on speculations which can be neither proved nor disproved. There is evidently a great deal of truth in this view, but, on the other hand, however profitable such an attitude may be for the general progress of granite geology and petrology, there are still a large number of really dependable facts in this field, even though some of them have been considered comparatively seldom in this connection.

By putting the word "granite" of the title of his paper in quotation marks, Walton probably indicates one of the most serious reasons why opinions regarding both the emplacement and the origin of granite are so many and so controversial. There are "granites and granites" (Read, 1947), but rocks are often included among the "granites" which have nothing to do with granite in the petrological meaning of this term. The present paper aims to draw the attention of petrologists particularly interested in granite petrology to certain facts, often observed either in the field or under the microscope but rarely considered in the literature dealing with granite questions, that undoubtedly have an important bearing on the problems of emplacement and origin of granites.

"GRANITES" AND GRANITES

First of all: What is granite? The *Concise Oxford Dictionary* says that granite is a "granular, crystalline rock of quartz, feldspar and mica, used for

building." This definition is perhaps more limited than that of most petrologists. Johannsen (1949, p. 124) gives for granite a strict limitation: "The rocks (granites) are characterized by quartz forming more than 5 and less than 50 per cent of the quarfeloids, and by a feldspar ratio such that Kf forms from 95 to 50 percent of the total feldspar content. The plagioclase is CaNaf, and the mafites form more than 5 and less than 50 per cent of the total constituents." But on reading various descriptions of granites from around the world, Walton's remark (1955, p. 1, footnote): " . . . it is tacitly assumed by longstanding custom that the word granite means the whole class of quartz-feldspar rocks of plutonic habit, of which granite is in the strict sense a somewhat subordinate member" seems to portray the prevalent conditions excellently.

If for a moment we forget that only a minority of all rocks described as granite are true granite, we encounter an enormous literature dealing with the "granites"; and in this literature, since the very beginning of modern petrography, we find a distinct tendency to divide the granites into groups, according to either their chemistry (Rosenbusch: Alkaligranit and Alkalikalkgranit), their age relations (Sederholm: I to IV group), or their place in the orogenic environs (Stille, Eskola, Wahl, Read, and many others). The present author adopted in his granite investigations the classification proposed by Eskola (1932) dividing the granites into syn-, late- and post-kinematic. In the geologic terrain of Finland such a classification can be applied comparatively easily, and likewise in Sierra Leone where all granites so far studied can be interperted as either syn- or late-kinematic (Marmo, 1955a). Particularly in Sierra Leone, the detailed study of granites has given many interesting results, mostly so far unpublished, which form in the main the basis of discussion in the following pages.

If granites are still considered in the widest sense of the term, among them the extreme potassic varieties have always taken their own place in the petrochemistry of granites, and when it was still rather usual, a score of years ago, to construct differentiation series of the igneous rocks, these potassic granites always remained outside the magmatic differentiation curves including all other, more basic, igneous rocks. As shown by Simonen (1948) for the Svecofennide granites (Finland) and by Marmo (1955b) for the Precambrian granites of West Africa, these "ultrapotassic" granites are typical of the group of late-kinematic granites. The synkinematic granites on the contrary are in most cases granodiorites, often even quartz diorites. Hence they fall entirely outside the strict definition of the term "granite." Their content in potash feldspar is frequently so small that it can hardly be detected under the microscope. Even in the synkinematic group, however, a minority of potassium-rich members are often present, and they, naturally, are true granites both in the petrochemical and in the mineralogical sense. As shown below, the potash feldspar of these granites occurs in such a manner that its metasomatic origin seems to be without doubt, and recently Eskola (1954) definitely assumed that such granites are virtually granitized granodiorites. According to Eskola (1950), the most typical granites are to be found par-

ticularly among the potassic late-kinematic granites, and he designated them as "ideal granites."

There is an additional difference between the syn- and late-kinematic granites of the West African Precambrian orogenies—the composition of their plagioclase. The late-kinematic granites have rather albitic plagioclase (An_{10} or less), and among them, at least in Sierra Leone, both potassic and sodic varieties occur (Marmo, 1955b), and the albite is often accompanied by abundant epidote. In the synkinematic granites the plagioclase is oligoclase (An_{20} to An_{38}), and epidote is frequent but neither typical nor abundant. The late-kinematic granites are mostly muscovite granites; in the synkinematic granites biotite tends to predominate over muscovite, and in addition hornblende may often be present, though it is seldom met with in the late-kinematic granites.

Consequently, in actual fact the syn- and late-kinematic granites represent two entirely different kinds of rocks, and they should not be kept under the same indiscriminate heading of "granites." If discussion of granites continues to neglect the essential differences between these two groups, as has been done, for instance, in Perrin and Roubault (1950, p. 91, footnote), the results of discussion may be not only misleading but even wrong. Above all, the aforementioned "granite" groups should be distinguished and clearly understood. These groups have ample differences in their structural, textural, and stratigraphical features, very well known to petrologists for a long time, and regarding them one need only refer to the works of Eskola (1932, 1954), Wahl (1936), Backlund (1936, 1953), Read (1947, 1951), Saksela (1953), and others. Those authors who have accepted the duality of granites (Eskola, Backlund, Wahl, Read, etc.) also allow a duality in their origin. The extreme transformists, like Perrin and Roubault (1950), Termier and Termier (1953, p. 13), and some others, explain all granites as belonging to the same category, formed metasomatically from other rocks, without fusion.

Within the Precambrian "granite" areas seen by the writer, the synkinematic granodiorites and granites, both usually gneissose, are always extensively developed, but there are comparatively few uniform areas occupied by the late-kinematic granites. Most instructive, naturally, are areas where both syn- and late-kinematic granites occur, and such areas are abundant in south Finland and also occur in central Sierra Leone.

In each orogeny, both syn- and late-kinematic granites may occur (Wahl, 1936), and in most they probably do occur. We can distinguish with fair certainty between them within the same orogeny, but as far as known to the present author, comparisons of late-kinematic granites of different orogenies have so far not been made, at least of those of markedly different age. A comparison exists between old Precambrian granites of Finland and of West Africa (Marmo, 1955b), and the two groups are very similar, but what happens if a Tertiary and an old Precambrian orogeny are compared? According to field descriptions (King and de Swardt, 1949), even younger Precambrian Nigerian potassic granites seem to be petrochemically somewhat different from the central Sierra Leone late-kinematic granites, but no accurate comparison has been made there yet. As Walton (1955) remarked, Precambrian

and post-Cambrian granites may also differ in emplacement and origin. Comparative studies between such areas seem urgently necessary, therefore, from the point of view of granite problems. Unless such studies are made, the granite problem as a whole is still unripe for large generalizations. It should also be borne in mind that conclusions based solely on laboratory study without close collaboration with field geologists in the respective areas may be dangerous.

The present paper deals only with granites of the amphibolite and epidote amphibolite facies. It is very possible that the interpretations adopted here are not at all apt for the corresponding rocks of the granulite facies, where new factors may be introduced that do not affect the "granites" and granites considered in this paper.

METASOMATIC, PALINGENETIC, MAGMATIC

To keep this paper concise, references to older investigations or opinions are avoided as much as possible, and most statements below are based on the experiences of the writer, obtained from both Finland and Sierra Leone.

Very large areas composed entirely of synkinematic granodiorites, granites, and gneisses are well known to petrologists, and nowadays probably nobody will deny their metasomatic origin. But in spite of that the same areas often contain elements which, as Walton mentioned, one can interpret as of any of the origins listed in the heading above.

In central Sierra Leone, a large schist belt forming the Sula Mountains and the Kangari Hills, 150 km long and 10 to 20 km wide, is entirely embraced by such synkinematic, "granitic" and gneissose rocks. In the middle part of the southern end of this schist belt, garnet quartzites occur, and the southward continuation of these metasediments is well shown within the synkinematic granodiorites and granites by a zone composed of garnet-bearing granodiorite and gneiss, often containing narrow stringers of well preserved or only slightly granitized garnet quartzite. In the north, the banded ironstone of the schist belt finds its narrow continuation within the gneiss area in the form of a strip of magnetite gneiss. The hornblende granodiorite usually envelops the amphibolite lenses within the gneiss area, and a zone of sillimanite quartzite in the south continues into the gneisses, there forming sillimanite gneiss.

From field observations there remains hardly any doubt about the metasomatic origin of these gneisses, but if one moves away from the schist belt, these usually strongly gneissose rocks grade little by little into more and more massive granodiorite and granite, ultimately quite homogeneous and frequently porphyroblastic.

Entirely similar granodioritic rocks, homogeneous and massive, occupy large areas in middle Finland. There they contain only dark inclusions but no immediate signs indicating their original material, and from their appearance a magmatic origin seems possible, if not evident, especially as homogeneous diorite occurs at Jyväskylä, 30 to 60 km southeast of the homogeneous granodiorite areas. If one follows the same rocks northwards, however, they become more and more gneissose, and approximately 50 km

northeast of Keuruu, where the granodiorite is especially homogeneous, the rocks contain stringers and larger bodies of cordierite gneiss. If now, an open-minded geologist approached the center of this large area from the southeast, he would probably explain these granodiorites as magmatic, but approaching them from the north he would perhaps adopt a metasomatic interpretation.

Small dikes, veins and veinlets are frequent within the synkinematic areas, and they are often entirely similar in composition to the rocks penetrated and brecciated by these veins. Such dikes and veins usually have sharp margins, and there is no sign of any "contact effects." They appear, however, in all respects to be injected. They are intrusive veins, but has the intrusion been "cold" or "hot"? There is no field evidence to decide for sure whether they are palingenetic, magmatic, replacement dikes due to ion migration, or, if monomineralic, products of precipitation from solutions.

What about the above-mentioned diorite at Jyväskylä, or a very similar, comparatively large diorite body at Tungie in Sierra Leone, 7½ km east of the aforementioned schist belt? Are they magmatic? All field evidence favors the assumption that they have been emplaced in a molten or plastic stage. At Tungie the diorite is very homogeneous, and it grades very rapidly if not sharply into embracing synkinematic granodiorites. But there is, then, another question, is this molten or plastic material of magmatic origin (in the strict sense) or has it been produced by palingenesis? At least at present, in the opinion of the present writer, to decide this question one would be stepping over the margin between theories based on actual observation and speculative assumptions.

This diorite occurs within an area occupied by synkinematic granodiorite and granite. Of an entirely different category are the smaller diorite bodies, often embracing amphibolite lenses; they grade into granodiorite on one side, less evidently into amphibolite on the other side. In hand specimens the two diorites are indistinguishable, but observations in the field clearly suggest a different origin for these similarly composed rocks.

Consequently there are "granites and granites" among the synkinematic granodiorites and granites also, and within a synkinematic area obviously all elements of the heading of this chapter—metasomatic, palingenetic, and magmatic—are represented in one manner or another.

Entirely different are the relations of the late-kinematic granites and, as shown below, they put us up against a really puzzling problem.

POTASH FELDSPAR OF THE GRANITES

The principal difference between granodiorite and granite is the larger potash feldspar content of the latter, and also to some extent an increasing amount of quartz. The presence of quartz is allowed also in diorites, which by increase of quartz turn into quartz diorite and, if small amounts of potash feldspar are added, into granodiorite. Consequently, within the synkinematic rocks, two different kinds of rocks may occur and usually do: those containing practically no potash feldspar (diorite and quartz diorite) and those containing it in varying amounts (granodiorite and granite).

As the presence of potash feldspar is clearly the distinguishing factor between two rock groups, one naturally tries first to determine how and when this new constituent was added.

Starting from hornblende granodiorite (or diorite), a small addition of potassium tends to turn its hornblende into biotite, thus resulting in biotite granodiorite (which may or may not contain muscovite in addition); further introduction of potassium results in the formation of potash feldspar, and the composition of the rock grades into granite.

If in line with the petrologic thought of 30 to 40 years ago granite were considered an end member of the differentiation of basaltic parent magma, there should be a continuous gradation from peridotite to granite. We know, however, that between granodiorite and Eskola's ideal granite there is a rather abrupt discontinuity, caused principally by a sudden increase in alkalies (predominantly potassium) and silica, either because of the tendency of potassium to accumulate entirely in the last rest liquids, producing the ideal granite magma (the still-remaining excess going into pegmatites), or because of an introduction of potassium from external sources into melts.

Eskola's opinion (1954) is that the break in the differentiation diagrams is as distinct as it is because granite intrusions are typical of a stage of orogenic evolution where there must be a much larger hiatus between the ideal granite and the previous differentiate than between any other two more basic members of the same series. Now the actual granite problem is bound up with the late-kinematic granites, but not to a similar extent with the synkinematic ones. At the moment, however, we are more interested in the potash feldspar of the synkinematic granites. There, at least theoretically, the microcline may be a primary component of the granite, or it may be introduced metasomati-

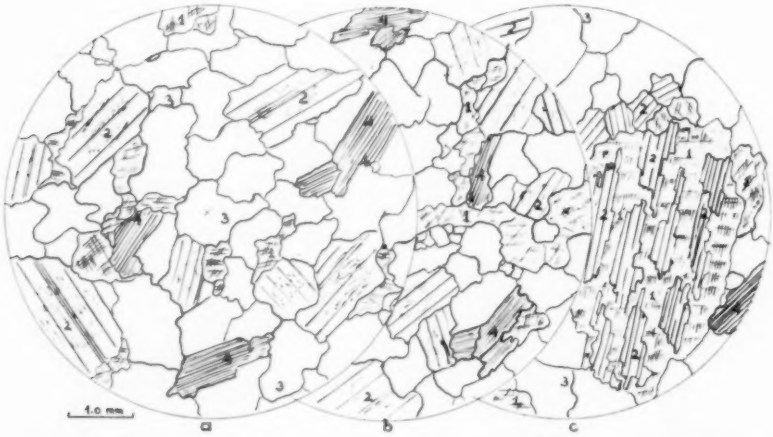

Fig. 1. Microcline in different synkinematic granites of central Sierra Leone: (a) microcline filling interstices; (b) microcline forming veinlets in granite; (c) microcline replacing plagioclase. 1—microcline; 2—plagioclase; 3—quartz; 4—mica. Thin sections; crossed nicols used only for feldspars.

cally into the rock. If it were a primary constituent, the potash feldspar should be distributed in the granite more or less homogeneously. But it is not; in the synkinematic Precambrian granites of middle Finland and of central Sierra Leone most of the microcline occurs either filling the interstices between other minerals, forming minute veinlets, or replacing the plagioclase (Marmo, 1955c). In figure 1 these occurrences are illustrated. There remains no doubt that the microcline in such rocks is the youngest constituent, mostly, if not exclusively, deposited within a solid rock. Such facts probably led Eskola (1954) also to conclude that most of the potash feldspar of the synkinematic granites has been metasomatically introduced.

Microcline occurs quite differently in the late-kinematic granites (see below).

Figure 1-c shows how sodium, replaced by potassium, has been redeposited on the ends of the replaced plagioclase in the form of minute, fresh grains of albite; such instances are well known to granite petrologists, and they have also been described by Marmo and Hyvärinen (1953) from Finland.

How the potassium moved into the rock and inside the rock is another question entirely. In general in geological literature, three possible interpretations have been adopted: (1) along the intergranular films (Wegmann); (2) in the form of pore magmas (Eskola); (3) by diffusion through the solid (Perrin and Roubault, Ramberg).

As pointed out by several authors (e.g., Rosenqvist, 1952), diffusion through solids is far too slow to allow any large-scale distribution of the potash feldspar. As a matter of fact, however, *in the synkinematic granites the distribution of potash feldspar does not appear to be very homogeneous, nor is it omnipresent*, but the majority of the synkinematic salic rocks consist of granodiorites. In central Sierra Leone, the synkinematic rocks rich in potash feldspar (excluding the porphyroblastic granites) are restricted to comparatively narrow zones, characterized by abundant shearing and pointing, often by faulting and shattering, and a great deal of evidence indicates that the faulting and shattering are older than the potash feldspar, which frequently fills the joints and fissures. Evidently, potash feldspar within synkinematic areas tends to be deposited at places corresponding to lower free energy than the environs, which are usually poor in potash feldspar—in good agreement with Ramberg's theoretically based statements.

In this connection it may be pointed out also that the granites of central Sierra Leone are very old. According to age determinations of Geiss (unpublished data), the age of galena from their pegmatites is more than 2.5×10^9 years. If that is the age of the corresponding orogeny, the potassium has had time to migrate quite a distance. If the rate of migration of a potassium ion is of the magnitude of 10^{-3} mm per annum, in 100×10^6 years the ion is able to migrate approximately 100 meters, and if the migration of all ions is directed by external causes, accumulation of potassium within a zone 100 meters wide is quite possible. As a matter of fact, *the potassium-rich zones on both sides of the above-mentioned shattered zones in central Sierra Leone are seldom more than ½ km wide, frequently much less.*

On the other hand, diffusion in the solid state need not have any outstanding importance, for along the shattered zones migration by other means is also possible, and often even easier. The occurrence of the potassium feldspar in the interstices in the potassic rock itself rather supports the idea of movements along the intergranular films. Introduction along the shear zones on the other hand can be explained best by Eskola's pore fluids, but they do not need to have anything to do with magmatic evolution. The replacement of plagioclase by microcline may undoubtedly be best explained by diffusion through solids, the distances being of the order of a few millimeters to a few centimeters.

The source of potassium is again another question. According to Eskola, the parent magma itself should contain potassium enough to supply it for all potassic granitic rocks. There is, however, potassium in abundance also in the sediments supposed to have granitized. Recent clay sediments (see e.g., Pettijohn) *may be richer in potassium than the rocks supposed to have originated from similar sediments by regional metamorphism.* Hence "granodioritization" seems to be a more typical feature of the synkinematic evolution of rocks than "granitization."

ORTHOCLASE OR MICROCLINE

Johannsen's statement in his textbook (1949) that the potash feldspar of the granites is predominantly orthoclase is entirely wrong for the old Precambrian granites corresponding to the amphibolite or a lower metamorphic facies, and just such granites form the majority of all syn- and late-kinematic "granite and gneiss" areas. The potash feldspar of such granites, both in the Svecofennide granites of Finland (Mäkinen, 1917) and the old Precambrian granites of West Africa (Swardt, 1953; Marmo, 1955b), are exclusively microcline, both in syn- and in late-kinematic granites and also in the corresponding pegmatites. In granulites orthoclase is often present, and there it may be the main potash feldspar (Eskola, 1952). According to Naidu (1954), however, the potash feldspar of the Indian charnockites is microcline. From the point of view of the emplacement and origin of granites, this fact, in the opinion of the present author, is of extreme importance.

Goldsmith and Laves (1954b) held the opinion that most microcline has been formed from a monoclinic form of potash feldspar, which corresponds to a higher level of free energy than triclinic microcline. Eskola (1952a) is also of the same opinion regarding the microcline of the granulites of Finnish Lapland. This opinion, however, encounters difficulties in the granites of central Sierra Leone, especially for the microcline of interstices or that replacing the plagioclase, which can hardly have been an orthoclase before. The matter is obscure because microcline has never been made synthetically. We know that at 525°C. in hydrothermal conditions (Goldsmith and Laves, 1954a) microcline transforms to a monoclinic modification, but on the other hand orthoclase forms at all temperature and pressure conditions that have been attained experimentally. The present writer has supposed that the formation of microcline is necessarily bound to *a temperature lower than that of the high amphibolite facies and to an extremely slow accumulation of*

material, consequently to conditions which are to be readily expected during synkinematic granitization.

PORPHYROBLASTIC GRANITE

Among the granites of the synkinematic areas, some contain large quartz and feldspar insets in a comparatively coarse groundmass, usually of granodioritic composition. In the insets microcline often predominates, giving the rock the bulk composition of granite.

In earlier papers such granites have often been interpreted as porphyritic and consequently as true intrusives. Microcline can hardly form phenocrysts, however, and in most later papers such insets have been considered porphyroblasts.

In the majority of specimens examined by the writer both in Finland and in Sierra Leone the matrix of the porphyroblastic granites, as already mentioned, is quartz dioritic to granodioritic, the microcline being almost entirely concentrated in the insets. Marmo and Toini Mikkola (in preparation) have studied the microcline of Sierra Leone granites, and they found that the microcline of such porphyroblasts has, like that of pegmatites, almost perfect triclinicity. At the request of the author, this was also checked by X-ray methods by Dr. Julian Goldsmith of the University of Chicago. The triclinicity is slightly higher than that of the interstitial or replacing microcline of the synkinematic granites. In all porphyroblastic granites so far examined the writer can prove without doubt that the microcline is younger than the matrix, especially in West Africa where such granites often occur in tor-like formations or in low, well rounded hills, occupying the cores of dome-like structures embraced by shells of strongly gneissose rocks of migmatitic character.

The distribution of the porphyroblasts being rather regular, one readily gets an impression of intrusive emplacement of such dome-like bodies. This intrusion, however, hardly took place in a molten state or by ordinary intrusion mechanics, but rather by the addition of potash feldspar and silica, both occurring in porphyroblasts, increasing the volume of the rock and, if the tectonic conditions were favorable, by a dome-like emplacement. On the other hand if the parent sediments were richer in potassium than granodiorite, such porphyroblasts could result from concretionary accumulation of the pre-existing potassium. These are, however, tentative hypotheses that need many additional observations to be either proved or disproved. Here these hypotheses are mentioned only to show how dangerous generalization in respect to the emplacement of granites can be, since undoubtedly the emplacement of porphyroblastic granite is unlike the "emplacements" of the synkinematic granites in general.

LATE-KINEMATIC GRANITES

The late-kinematic granites are entirely different in occurrence from the synkinematic granites and granodiorites, and their nature is often that of true intrusives.

Very often, perhaps in most cases, the late-kinematic granites have been described as injected bodies, and in the opinion of many leading geologists

(Bowen, Eskola, etc.), they are representatives of true granitic magma. This problem is not discussed here; instead an attempt is made to describe certain features in their textures, easily discovered under the microscope or in the field, which make the understanding of these granites rather difficult and force us to be very critical concerning the points of view so far presented as to their emplacement and origin.

The late-kinematic granites mostly represent a lower grade of metamorphism than the synkinematic granodiorites and granites. The plagioclase of the late-kinematic granites is usually albitic and they also contain epidote, not uncommonly 5 to 10 percent by volume; their mineral association is that of the epidote-amphibolite facies (or saussurite facies of Rosenqvist). This fact, and the additional fact that their potash feldspar is microcline, favors the hypothesis that their emplacement has taken place under hydrothermal conditions (providing albite and epidote are not primary but the result of saussuritization of plagioclase). If now an ideal granite magma exists, then according to the mineral composition of the late-kinematic granites this magma must have become solid long before its emplacement in its final locus, the upper stability temperature of microcline being too low. If the potash feldspar was primarily orthoclase, this conclusion is less valid. At present there are no means of proving what the original modification of the potash feldspar of the late-kinematic granites was, unless remnants of orthoclase can be found, yet in spite of careful searching they have never been seen in such granites. One may naturally say, and such an opinion has also been expressed, that the orthoclase has been completely altered into microcline. Why then did it not happen similarly in granulites, which are often of much greater age than the late-kinematic granites and may be cut by the latter?

The cores of mantled domes (Eskola, 1952b) are often made of late-kinematic granites. One structure probably closely related to the mantled domes has been described by Marmo (1955a) from central Sierra Leone. Walton (1955) mentions salt domes to explain the space problem, so closely involved in the granite problem. Concerning the Precambrian domes the space problem in most cases does not appear problematic, for they are mostly situated at tectonic culminations (see Eskola, 1952b), but the *mechanics* of salt domes may quite well be applied to the doming of *solid* granite. As Marmo (1955b) has shown, the late-kinematic granites have a larger volume than the synkinematic granites, as indicated by the volumes of their standard cells. This fact supports the ideas outlined above for the emplacement of the late-kinematic granites forming the cores of the mantled domes.

The potash feldspar of the late-kinematic granites occurs in an essentially different manner from that of the synkinematic granites, seldom forming interstitial grains or replacing plagioclase, but usually occurring as well formed grains in all respects comparable with albite and also quartz (fig. 2). Hence the relative age of microcline is somewhat doubtful in the late-kinematic granites, whereas it was not in the synkinematic granites, and one rather gets an impression of simultaneous formation of all salic constituents, which probably favors the magmatic origin of the late-kinematic granites. This appearance of microcline, and its presence instead of orthoclase, is the

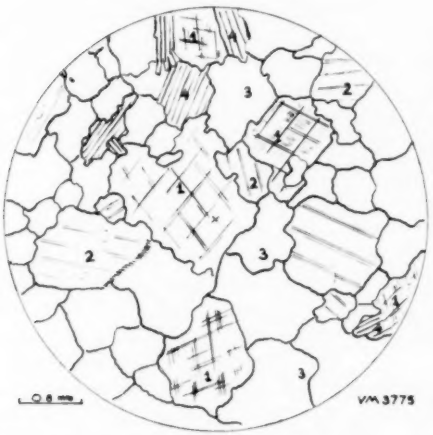

Fig. 2. Late-kinematic granite. 1—microcline; 2—albite; 3—quartz; 4—muscovite. Thin section; crossed nicols used only for feldspars.

focal point of discussions concerning the emplacement and origin of granites, particularly of the late-kinematic granites.

It is noteworthy, however, that even among late-kinematic granites varieties poor in potassium may occur, and they consist largely of albite, quartz, epidote, and muscovite. In extreme cases they are "albite-epidote granites," the "helsinkites" of Laitakari (1918), which could better be named "albite granodiorite" or "granodiorites of the epidote-amphibolite facies." Furthermore, these late-kinematic albite rocks differ markedly from the corresponding synkinematic oligoclase granodiorites, being richer in total alkalies (fig. 3). Unfortunately these late-kinematic subgroups have so far been studied comparatively little. In central Sierra Leone, potassic and sodic late-kinematic rocks occur in a very similar manner, yet the existence of the two late-kinematic petrochemical groups immediately raises the question whether, among the late-kinematic granites of old Precambrian orogenies, "granites" and granites also exist.

Post-kinematic granites have been described from south Finland but not so far from central Sierra Leone. In their petrochemistry they are indistinguishable from the late-kinematic granites, being consequently ideal granites. It seems that these post-kinematic granites are the disharmonious granites of Walton (1955). Accordingly, discussions of late- and post-kinematic granites should be clearly detached from discussions concerning the "emplacement and origin" of the synkinematic rocks.

The large quantity of the granitic rocks in certain areas has often been emphasized, yet in the large "granite" area of central Sierra Leone and in those of central Finland the synkinematic *granodiorites* predominate. In the vast area of central Sierra Leone the late-kinematic granites occupy hardly more than 60 sq. km, but the area occupied by the synkinematic granodiorites and granites amounts to at least 6000 sq. km. Among these synkinematic

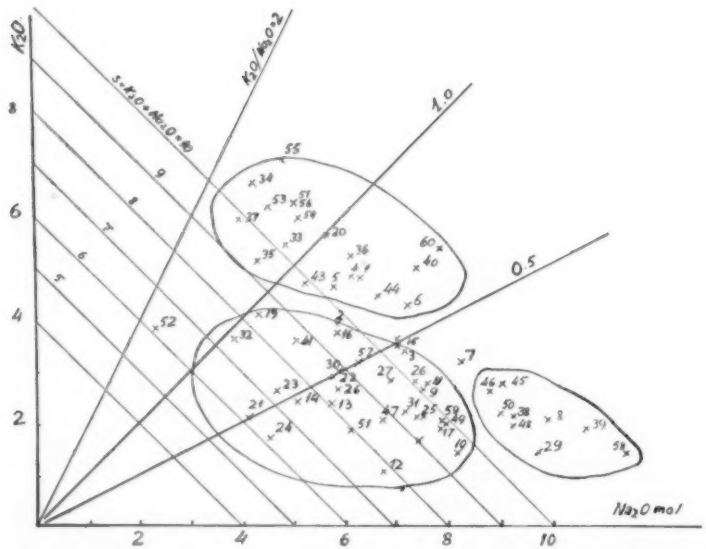

Fig. 3. Variation diagram of granitic rocks of West Africa. The rocks corresponding in their petrochemistry to the synkinematic granodiorites and granites occupy an area around the ratio $K_2O:Na_2O = ½$ and have total alkalies predominantly less than 10 moles (diagonal $S = K_2O + Na_2O = 10$ of the graph). The granites corresponding in their petrochemistry to the late-kinematic granites have total alkalies predominantly more than 10 moles but split definitely into two groups: those having $K_2O:Na_2O$ between ½ and 2 and those definitely below ½.

rocks, excluding the porphyroblastic granites, the true granites form a few percent only. Consequently the main problem there is the formation of granodiorite, not of granite; the actual difficulties, however, are bound to the interpretation of the origin and emplacement of granites—that is, of rocks rich in potassium.

CONCLUSIONS

Before dependable theories concerning the problems of emplacement and origin of granites can be established, the rocks called "granite" must be subdivided. The term "granite" should be applied only to true granites, mainly late- and post-kinematic, which approach the ideal granites of Eskola, and in which the potash feldspar is paragenetically equivalent to the other rock-forming minerals. These granites should definitely be studied and discussed apart from other "granitic-looking" rocks, even though they are in the minority in the large "granite" areas.

Discussing the synkinematic granitic-looking rocks actually means dealing with entirely different problems, but here subdivision is also necessary. There are large areas where the predominant synkinematic rock is granodiorite and not granite. Consequently the formation of granodiorite, "granodioritization," is the main problem in such areas and not "granitization."

Granitization means metasomatic change in the composition of a rock leading to granite, and hence involves incorporation of potassium. The granodiorites, on the contrary, often contain less potassium than the clay sediments that have frequently been considered as the primary material of the "granites."

The synkinematic granites (rocks rich in potassium) differ from the late-kinematic granites in the manner of occurrence of their potash feldspar. Mostly they are granitized granodiorites, and as such they must be described and discussed quite separately from the true granites discussed above.

Furthermore, among the synkinematic rocks porphyroblastic granites always occur. They have the bulk composition of granite, but their potash feldspar is concentrated entirely into porphyroblasts, the matrix being usually of granodioritic or even dioritic composition. The discussion of these granites presents a new petrogenetic problem separate from those dealing with other aforementioned rocks of granitic composition.

In the opinion of the present author, the consideration of the kind of potash feldspar present in the granites is of first importance. The fact that most Precambrian granites contain microcline, which is not stable at higher temperatures under hydrothermal conditions, needs careful examination, and if any theories regarding the origin of granites are made this feature must also be explained.

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CAVITATION AS A GEOLOGICAL AGENT

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ABSTRACT. Cavitation is the formation and collapse of bubbles of vapor in a fluid stream. Formation of the bubbles is caused by a local reduction in absolute pressure to the vapor pressure of the fluid; collapse of the bubbles is caused by a subsequent pressure increase. The pressure reduction is usually caused by an increase in local velocity due to an irregular streamline configuration (Bernoulli's Theorem). The mechanism is important because the powerful shock waves initiated by the collapse (i.e., implosion) of the bubbles cause rapid erosion of even the hardest materials. For example, cavitation eroded to a maximum depth of 18 inches of concrete in a dam spillway in 23 hours.

Although many complex variables affect cavitation, each is sufficiently well known to predict natural occurrences on a semi-quantitative basis. In an open stream, cavitation would be expected to begin at velocities of 25 to 30 feet per second.

Cavitation may be an important agent in the erosion of rock by glacial meltwaters and by high velocity streams. In a hydrothermal environment, cavitation is probably only a very rare phenomenon and of little importance.

INTRODUCTION

The purpose of this paper is to bring to the attention of geologists the mechanism and occurrences of cavitation and to suggest its possibilities as an agent in diverse geological phenomena.

In most phases of hydraulic engineering cavitation presents a serious problem; therefore, much pertinent material on the subject may be found in engineering publications. With the notable exception of Hjulström's paper (1935), geological papers in general do not mention cavitation. In the following discussion, the mechanism of cavitation in water is examined in reference to stream erosion. The principles are readily applicable to other fluids and to other processes, a few of which are considered superficially in the conclusion.

The writer gratefully acknowledges the advice and encouragement of Professors A. N. Strahler, C. H. Behre, Jr., and Sidney Paige of the Geology Department and Professor Richard Skalak of the Civil Engineering Department of Columbia University for critically reading the manuscript and suggesting several improvements. Thanks to the kind cooperation of the Bureau of Reclamation, the author was able to discuss cavitation with the engineers of the Bureau and to observe the experimental facilities of the Bureau's Laboratories in Denver.

BASIC MECHANISM

Elementary studies of fluid flow show that there is a reduction in pressure with an increase in velocity (Bernoulli's Theorem) in a fluid stream. If the velocity is sufficiently great, the pressure drops to the vapor pressure of water and "steam" can form. This "steam" or water vapor occurs as small bubbles, a fraction of a centimeter in diameter, which form at the point where this critical pressure is reached. These bubbles are rarely formed individually but usually occur as aggregates, giving the water the appearance of foaming. The bubbles may then either be carried away in the stream or become momentarily lodged against the walls or any solid material. In either event, if there is a slight increase in pressure the bubbles are then unstable and collapse very quickly and violently. The collapse of a bubble in contact with solid material produces a hammer-like blow of great strength on a very small area. If the

bubble collapses in the stream, violent shock waves are produced; these rapidly dissipate unless they come in contact with solid material, where they may pound on the surface. At present the experimental difficulties involved in the measurement of the instantaneous and very local pressure from a bubble collapse have not been conquered, but experimental results recently gave 30,000 atmospheres as a minimum pressure (Vennard and Lomax, 1950). The large order of magnitude of the pressure is very significant for the understanding of cavitation erosion. The effect of these "hammer blows" is to shatter the surface of any nearby solid material; then pieces become dislodged and are carried away as the fracturing continues and surface roughness increases. The erosion of the viewing windows of experimental cavitation chambers is described by Rasmussen (1949, p. 44): "The glass is first penetrated by a fine network of cracks, then particles of the surface come loose, and gradually the glass assumes a 'frosted' appearance."

The rate of erosion is generally dependent on the smoothness of the walls; the smoother the walls, the greater the resistance to cavitation. The rate of erosion would therefore accelerate as erosion continued, because of the increased roughness. This increase in the rate of erosion has been noted in engineering projects which have become suddenly threatened by cavitation. In an extreme example, concrete exposed to cavitation in a dam spillway during one test eroded to a maximum depth of 18 inches in 23 hours (Ball, *in* Discussion of Symposium, 1947, p. 92).

Rasmussen (1949, p. 40) states that some erosion occurs also at the point of formation of the bubbles, but other authors do not concur and, if such erosion does take place, the cause is in doubt. The cause of the erosion of the walls at the area of collapse of the bubbles is now generally considered to be primarily mechanical and is directly due either to the pressure waves or to the collapse of the bubble in contact with the walls (Rasmussen, 1949, and Vennard *in* Symposium, 1947). Vibration induced by successive impacts probably causes appreciable failure through fatigue. A minor electrical and chemical effect is also known, and there may be other unknown effects. A temperature rise has been noted at the collapse of individual bubbles. The temperature at the collapse of a bubble has been calculated at 2700° K (Rutenbeck, 1941), and cavitation erosion with the appearance of fusion in test samples has been cited (Nowotny, 1942; Rutenbeck, 1941) as proof of the high temperatures at the point of collapse. The author has observed carbon-coated gobular forms due to fusion on an iron surface and the odor of burning rubber where each has been exposed to cavitation. The temperature has not been measured experimentally because, like the high pressure, the high temperature is only very local and instantaneous. This local high temperature at the collapse of a bubble would tend to form another bubble by evaporating more vapor. High speed motion pictures show a cyclic collapse and regrowth of bubbles (with decreasing size downstream) (Knapp, 1952); therefore, the temperature increase is probably important in increasing the number of impacts from bubble collapse by reforming collapsed bubbles.

Fig. 1. Cavitation in water flowing through a Venturi tube. (1) Area of minimum pressure and most rapid bubble development (foaming). (2) Area of greatest erosion (variable with velocity, pressure, etc.) caused by the statistically greatest number of bubbles collapsing.

Note: Cavitation may be produced in the laboratory by water flowing through a small constricted glass tube. A 5 mm glass tube constricted to about 2 mm and connected to a faucet by rubber tubing produces visible cavitation if the water flow is adjusted for the maximum effect. Essentially the same effect may be attained by gently squeezing a rubber tube with water flowing through it.

FACTORS AFFECTING BUBBLE FORMATION

Cavitation may be divided into two types called "burbling cavitation" and "laminar cavitation" (Edstrand, 1946). Burbling cavitation is characterized by simple groups of bubbles whereas laminar cavitation is characterized by long, pipe-shaped eddies with pipe-shaped bubbles at the axes of the eddies. The two types of cavitation may occur together, one or the other type predominating.

Because the erosion of the walls near the bubble collapse is essentially due to the implosion or shock waves from the sudden filling of the vacuum previously filled by the vapor, anything that would tend to reduce the incompressibility of water would reduce both the impact of the bubble collapse and the efficiency of transmission of the shock waves by acting as a "cushion." Air entrapped in water, either dissolved or suspended as bubbles, is drawn into the low pressure centers and enters the cavitation bubbles. The air in the bubble acts as a cushion when collapse does take place. According to Rasmussen, (1949, p. 56) about three-tenths of one percent air by volume reduces cavitation erosion to a minor or negligible phenomenon, regardless of the type of cavitation involved. The explanation is readily apparent when the compressibility of water is considered. A change in pressure causes an inversely proportional change in volume; for a one atmosphere change in pressure, the ratio of volume increment to original volume is 1 to 20,000 for pure water, but the ratio for water with 0.1 percent air by volume is 10 to 20,000 or ten times the compressibility of pure water (Rasmussen, 1949, p. 4).

Natural stream water contains various amounts of oxygen, nitrogen, argon, and carbonic acid (Hjulström, 1935, p. 307), but probably not in sufficient quantities under most conditions to reduce cavitation appreciably in a homogeneous stream. Experimental measurements have been made of the internal bubble pressure; pressures higher than the vapor pressure have been noted and interpreted as bubbles of gas taken from solution (Vennard *in* Symposium, 1947), but the usual measurement matched the vapor pressure very well. The close correlation between measured pressure and vapor pressure proves that the content of the bubbles is nearly all water vapor (Knapp and Hollander, 1948, p. 422).

For a slight increase in pressure to cause the violent collapse of the vapor bubbles in cavitation, there must be essentially instantaneous attainment of equilibrium between the vapor and liquid phases after a pressure change.

The slower the rate of attainment of equilibrium, the more the bubble collapse is cushioned and the less violent is the shock wave produced. The rate of approach to equilibrium depends on the difference between the vapor pressure and absolute pressure (other factors remaining constant). In other words, the more unstable the vapor under normal stream conditions the faster and more violent the collapse. It is also more difficult (i.e., it requires higher velocity) to form vapor bubbles under these conditions. Consequently, higher vapor pressure has the dual effect of increasing the number of vapor bubbles formed (Nowotny, 1942, p. 17) and decreasing the violence of each collapse. Since the rate of erosion depends on both the violence of each collapse and the number of bubbles collapsing (temporarily ignoring other factors), it is evident that there is an optimum range of vapor pressures at any specific absolute pressure for maximum rate of erosion. The vapor pressure is fixed by the temperature; therefore, we shall refer to the temperature instead of vapor pressure, because it is the more easily measured variable.

For a pressure of one atmosphere, Nowotny (1942, p. 12) gives the optimum temperature range of 55°-70°C.(131°-158°F.), but the efficiency of

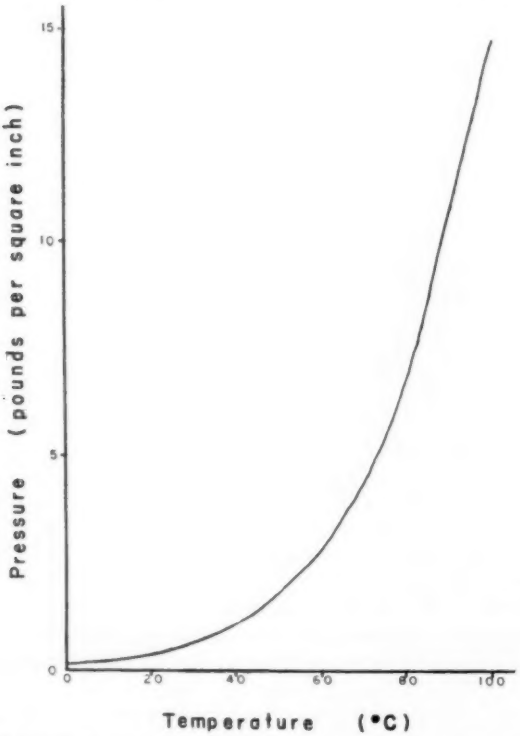


Fig. 2. Relation of vapor pressure of water to temperature. (Data recalculated from Rouse, 1950, p. 364.)

cavitation falls off rapidly only as the absolute pressure is approached by the vapor pressure, i.e., near 100°C. Probably the main reason for the reduction of erosion efficiency at high temperatures is simply that vapor pressure is an exponential function of temperature (fig. 2); at higher temperatures the vapor pressure increases more rapidly with temperature and reduces more rapidly the gradient between the vapor and absolute pressures.

Evidently the vapor pressure of the water is of primary importance to the formation of the bubbles, because the pressure at some place in the stream must drop to this critical pressure before water can convert from the liquid to the gaseous state and form bubbles. Figure 2 shows the variation of vapor pressure with temperature. Atmospheric pressure in general decreases about 0.45 pounds per square inch per thousand feet increase in elevation. The optimum condition for cavitation, with the highest vapor pressure within previously discussed limits and lowest atmospheric pressure, occurs at high temperatures and at high elevations. In other words, the conditions that require the least pressure reduction through increase of stream velocity, in order to form the vapor bubbles, are at high altitudes and high temperatures, although cavitation does occur more commonly under the less favorable conditions at sea level and relatively low temperatures.

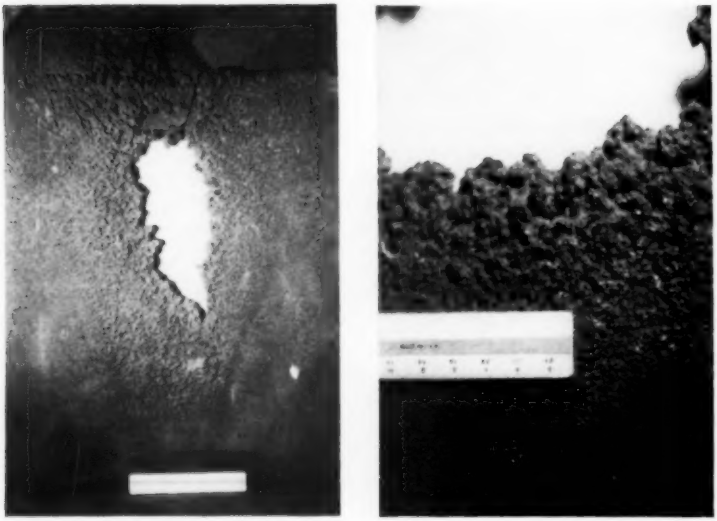
The pressure of only a small part of a stream ever drops to the vapor pressure. The actual triggering of bubble formation takes place at a constriction or a bend in stream flow lines where the velocity is increased for a short distance. A low pressure area is frequently noted at the wall on the concave side (in plan view) of the stream as, for example, at the wall near constrictions. Low pressures also occur at the center of eddies. Diagrams of several low pressure areas for typical stream line configurations, have been presented by Vennard (Symposium, 1947, p. 3).

Other properties of water which have a minor effect on the bubble formation are surface tension, viscosity, and heat of vaporization. Higher surface tension and viscosity oppose the formation of large bubbles, but high surface tension also increases the violence of the bubble collapse (Nowotny, 1942). Very high viscosity tends to prevent all cavitation because there is a hydrodynamic tendency towards "sheeting" in flow around an obstacle, and no vapor bubbles are formed. A high heat of vaporization hinders the bubble formation because a greater concentration of energy is necessary to form each bubble but the collapse would probably be more violent with the release of more energy. The properties of surface tension, viscosity, and heat of vaporization together are insignificant in magnitude of effect on bubble formation in comparison with the effect of vapor pressure (Nowotny, 1942.)

FACTORS AFFECTING STREAM VELOCITY

Since the viscosity of water decreases with increasing temperature, it is obvious that the same stream will flow with greater velocity when warm than when cold. Eisenlohr (1948) has shown that this effect is measurable on the Kootenai River in Idaho. Although the mean stream velocity increases with decreasing viscosity, the magnitude of the velocity change is very small and is insignificant in comparison with the magnitude of the effect of temperature

PLATE 1



A B

Cavitation erosion of a steel plate.

A. A hole through a 5/8 in. steel conduit segment from the Ross Dam outlets. Note that the underlying patch is also pitted. The direction of flow was vertical from top to bottom. The scale is 6 in. long. (Photo taken at the Bureau of Reclamation Laboratories, Denver, Colorado.)

B. Detail of the globular forms on the eroded steel surface of A. Numbered divisions on the scale are inches above and half-inches below. (Photo taken at the Bureau of Reclamation Laboratories, Denver, Colorado.)

on the vapor pressure in considerations of cavitation over short distances. It should be noted, however, that with increasing temperature the decrease in viscosity and the increase in vapor pressure have an additive effect in promoting cavitation in a stream.

A limitation on the minimum size of a high velocity stream is necessary for the occurrence of cavitation. The stream must flow as a homogeneous fluid body without breaking up into streams of drops. For example, a high waterfall of small discharge will often break up into drops while a waterfall of larger discharge will maintain an undispersed fluid stream. The maximum velocity of a dispersed flow is approximately the same as the maximum velocity of falling drops, i.e., about 25 feet per second for still air. The maximum theoretical velocity for open channel flow in contact with the banks and bottom and flowing under one atmosphere pressure is about 73 feet per second (Hjulström, 1935, p. 290). As shown below, cavitation probably does not often take place at velocities as low as 25 feet per second but would most certainly take place in any stream near a velocity of 73 feet per second, unless the walls were extremely smooth and regular.

The cross-sectional velocity distribution of a stream of very high velocity ("shooting flow") is fairly uniform except near the margins where a thin layer of laminar flow takes place. The bed roughness in most natural streams is important in that the projections from the rough bed pass through the laminar zone into the area of shooting flow, causing a sharp change in streamline configuration which may trigger bubble formation. A hemispherical projection into the stream flow causes a local velocity (at a point where the tangent plane is parallel to the normal stream lines) approximately 1.5 times the mean stream velocity (Rouse, 1950, p. 32). Other shapes of projections may cause local velocities greater than 2.4 times the mean stream velocity. This large variation in possible local velocity emphasizes the importance of the streamline configuration and its ability to increase the velocity at a point where cavitation might occur to more than 240 percent of the mean stream velocity.

In order to give an estimate of the channel slope required for the velocities necessary to produce cavitation, the following approximate calculations based on Bernoulli's equation are presented. This equation is not exact for this type of flow, but it gives results of the correct order of magnitude. The following conditions are assumed:

velocity distribution: constant velocity over the normal stream cross section

streamline configuration: local velocity at cavitation point (v_2) = 2 x mean stream velocity (v_1)

water temperature: 70°F. (vapor pressure = 0.36 lbs. per sq. in.)

surface pressure: 14.7 lbs. per sq. in. (sea level atmospheric pressure)

air content of water: insignificant

stream flow: as a homogeneous fluid (undispersed)

$$\frac{v_1^2}{2g} + \frac{p_1}{\gamma} + z_1 = \frac{v_2^2}{2g} + \frac{p_2}{\gamma} + z_2$$

v_1 = mean stream velocity

p_1 = absolute pressure = 14.7 lbs. per sq. in.

z_1 = elevation of stream surface (stream depth)

v_2 = velocity at cavitation point (channel bottom) = $2v_1$

p_2 = vapor pressure = 0.36 lbs. per sq. in.

z_2 = elevation of stream bottom = 0 (assumed datum)

γ = specific weight of water = 62.4 lbs. per cubic ft.

g = acceleration of gravity = 32.2 ft per sec^2

Converting all terms to ft-lbs.-sec units and evaluating:

$$\frac{14.7 \times 144}{62.4} + z_1 = \frac{3v_1^2}{2g} + \frac{0.36 \times 144}{62.4}$$

$$v_1 = 4.6\,(33.1 + z_1)^{1/2}$$

This equation shows the relation for the velocity necessary for cavitation to occur at any particular depth under the conditions assumed above. Note that this velocity is the mean velocity necessary for the stream flow since the local velocity due to the streamline configuration has been assumed equal to twice this mean velocity.

The derived equation gives the following conditions for the initiation of cavitation:

Depth (ft)	2.9	15.9	30.9	47.9
Velocity (ft per sec)	27.6	32.2	36.8	41.4

For water to attain a velocity of 27.6 ft per sec, a free fall of only 12 feet is necessary, neglecting air resistance. While these are obviously high velocities, they are not uncommon and appear quite often in falls and rapids (Hjulström, 1935, p. 311; he also states that the minimum velocity necessary for cavitation to occur in a stream is about 12 meters per second (39 ft per sec), but he does not give an adequate derivation or explanation of this figure).

Because there seems to be no widely accepted stream slope equation applicable to these velocities, it will be possible to give only an indication of the slope required for a velocity sufficient for cavitation. Manning's equation and the later modifications of it are not applicable, but an empirical equation derived by Ehrenberger (1926) and recently verified experimentally by Hedberg (1942), is applicable to steep slope flow. This equation holds for stream slope angles with the sine greater than 0.153 and less than 0.707 (approximately 9° to 45°). Because the equation was based on experiments on flow through an inclined flume of planed wood, the bed roughness of a natural stream introduces an error that is assumed negligible for the following approximate calculation.

By assuming appropriate dimensions for a natural stream, the velocity corresponding to the slope at the lower limit of the range of applicability of the equation may be calculated as follows:

Average width: 12 feet

Average depth: 3 feet

Hydraulic radius (r): $\dfrac{\text{cross-sectional area}}{\text{wetted perimeter}} = \dfrac{36}{18}$

Sine of slope angle (s): 0.153 (approximately 9° slope)

Mean stream velocity: v

$$v = 97\, r^{0.52}\, s^{0.40}$$

$$v = 97 \left(\frac{36}{18}\right)^{0.52} (0.153)^{0.40}$$

$$v = 65.6 \text{ ft per sec}$$

Although the channel roughness may be ignored for shooting flow (Hjulström, 1935), the curving of the stream channel would lower the velocity to some degree below the value indicated by the above calculation. The magnitude of the effect of this factor may vary widely depending upon the characteristics of each stream.

The above calculations have shown that cavitation may occur at 27.6 ft per sec at a depth of 3 feet and also that a slope of 9° is sufficient for a velocity of 65.5 ft per sec in a stream 3 feet deep by 12 feet wide. It is reasonable

to expect cavitation under favorable conditions in a stream of this size with a slope somewhat greater than one-half of the 9° slope.

It should be pointed out again that many factors such as the cross-sectional velocity distribution and the amount of dissolved air have had to be neglected in the calculations because of lack of data or complexity, and often both, but they are important when one is considering the possibility of cavitation erosion at a specific point in a stream.

The mean velocity has been used in these calculations for simplicity, but cases may easily occur where there is no cavitation at the mean velocity but cavitation does occur at the maximum velocity, although calculations would indicate that cavitation erosion is unlikely. There is a velocity variation over short time intervals as well as in cross-sectional distribution in streams. Turbulence and various streamline configurations also contribute to the difference between the mean and maximum velocities. Proof of this variation is easily obtained. Since it is easy to detect the very audible noise of cavitation, sound is a better criterion than sight for the inception of cavitation especially if the occurrence is sporadic in both time and location. Intermittent cavitation noise without visible evidence is common for mean velocities slightly lower than those which calculations indicate as necessary for cavitation. Irregular variation in the noise level, which is more or less proportional to cavitation violence, also indicates the variation in velocity with time for continuous cavitation conditions. Under most conditions, the velocity fluctuation would not be great but it would be sufficient to allow some erosion at favorable locations at lower velocities than calculations suggest.

The author has been unsuccessful in attempting to find reliable data on the air content in the water of high velocity streams. In 1926, Ehrenberger published an equation relating air content of a stream flow to hydraulic radius and slope, but the result of calculations based on these formulas does not seem to be verified by observations of actual conditions. Although the air content of streams may generally be assumed low, only future data can confirm the assumption.

The shape of the erosional form caused by cavitation is highly varied. The area producing the bubbles, the length of the low-pressure area, the average bubble path, the variation in pressure with time, and the wall shape are all factors affecting the erosional form. The depth of the erosion at a given point depends statistically on the number of the bubbles collapsing at that point. The great variation in the factors mentioned above makes it evident that no standard shape can be ascribed to cavitational erosion forms.

CAVITATION IN NATURE

Because cavitation erosion does not depend on the abrasive properties of transported material, erosion by glacial meltwaters is susceptible to explanation by this mechanism. The potholes found so frequently near and at the margin of former continental ice sheets may well be considered. In studies of these potholes by Elston (1917, 1918), Alexander (1932), and others, the formation of the potholes has been attributed to many mechanisms. They are found in a great variety of rock types and with many shapes and sizes up to

PLATE 2
Cavitation potholes (Mt. Tecumseh, New Hampshire)

A. Water-filled cavitation potholes. The pick handle points upstream.

B. A cavitation pothole deepened by abrasion of rotating pebbles. A shallow shelf below the pick and on the near side indicates an original shape like the lower pothole in A above. Upstream was to the right.

12 feet in diameter and 60 feet deep, as at Taylor's Falls, Minnesota. Many of the potholes cannot be related to any stream channel, yet a stream channel would need a long interval of time to erode such a volume of rock (using normal erosional mechanisms) and would have to form a stable channel leaving obvious geological evidence of its presence and continuity. The erosional mechanisms proposed by Alexander are: (1) rotary action of water with abrasive rock fragments (producing "eddy holes"); (2) direct impact of water and abrasive rock fragments (producing "gouge holes"); (3) falling water and rock fragments (producing "plunge-pool holes"). Each of these processes would not normally start a deep hole of the volume of many potholes without the action of a major stream channel over a long period.

Cavitation may well be the mechanism that started the formation of these potholes. The volume and velocity of glacial streams is often great, but they are also extremely variable in time. Because cavitation is effective under these conditions and acts very quickly, a shallow hole could form rapidly, and natural erosion by any of the processes named above could soon form a pothole. A hole in a stream bed tends to enlarge itself by trapping boulders and pebbles which are rotated by the stream and abrade the sides of the hole. The supply of rock fragments and debris must not exceed the rate of erosion in the pothole or the hole would soon be filled.

Hjulström (1935, p. 318) refers to several forms and gives a picture of one form observed near glaciers in fjords near Oslo. These forms are of the general shape which could be expected by cavitation erosion. He also refers (p. 319) to descriptions and pictures from other authors which may well be cavitational forms. The abrasion by rotation of rock fragments would destroy the cavitation shape in a short time so the forms mentioned above may well be very short-lived features under most conditions (pl. 2). To preserve the cavitation shape, the normal erosion processes, which would form a pothole, must be arrested before the transition of the principal erosional mechanism from cavitation to abrasion.

The mechanism of cavitation[1] may be applicable to hydrothermal conditions. In general, fluids not in a hydraulically closed system cannot transmit mechanical forces, but cavitation can produce mechanical force in an open system without excessive fluid velocities if the temperature is high enough to have the vapor pressure near the absolute pressure. A study of the conversion of the kinetic energy of the fluid flow to the potential energy of the phase change and back to kinetic energy again in shock waves could show how large this effect might be in hydrothermal conditions.

Cavitation might produce a fine shattering of wall rocks in hot spring areas that would aid the penetration of altering solutions; one might thus account for some of the fine fractures in some ore specimens, but cavitation would not generally be expected under these conditions. Volatiles in the fluid

[1] Although the word "cavitation," strictly speaking, postulates the same chemical composition for vapor and liquid phases, there may be a marked difference in composition between vapor and liquid in hydrothermal conditions. Because the mechanism is essentially unchanged, it will be referred to as cavitation in this paper regardless of the composition of the phases.

would tend to form permanent bubbles that would expand with decreasing pressure on approaching the surface rather than collapse violently. The gas content of these bubbles would also act like an air cushion during the collapse. Finally, the fluid would either have to flow at high velocity or be at a temperature near the boiling point. In the latter case the energy released from the bubble collapse may be insignificant.

CONCLUSION

Cavitation is probably an important geomorphological agent under specific conditions where water flows faster than 25 feet per second. Some types of potholes, especially those of glacial origin, may be started by cavitation eroding a shallow hole one to several feet in plan dimensions. The erosion takes place downstream of an irregularity in the channel where streamlines diverge from the channel sides or bottom. Later, rocks trapped in the hole may deepen and enlarge it by abrasion during rotation by the water flow.

In hydrothermal conditions, cavitation would be expected only very rarely, and evidence of its action would easily be destroyed by later alteration or replacement.

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* Note: Excellent bibliographies of references on cavitation may be found in the Symposium (1947) and in Eisenberg's paper (1950).

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THERMOMETRIC AND PETROGENETIC SIGNIFICANCE OF TITANIFEROUS MAGNETITE—DISCUSSION

KNUT HEIER

The September 1955 issue of this Journal brings an article about "Thermometric and Petrogenetic Significance of Titaniferous Magnetite," by Buddington, Fahey, and Vlisidis. The authors, in the conclusions of their paper, warn against making interpretations of the data presented. This does not, however, prohibit them from making the following statement in their abstract: "*The data serve to re-emphasize that the formation of granophyre, microperthite granite, and most titaniferous magnetite-ilmenite deposits are at magmatic temperatures.*" This rather sweeping statement made me think over the problem, and after having discussed the matter with Drs. T. F. W. Barth, H. Neumann and H. Carstens, I should like to offer the following statements:

Buddington et al. estimate the temperature of formation of magnetite in basaltic lavas at around 1000°C (based on actual temperature measurements of lava flows). For the more acid, dacitic, lavas they accept temperatures between 750-900°C. These temperatures may be correct for lavas but do not apply to magmas (= subterraneous lavas).

Buddington et al. are, more or less directly, transferring temperatures deduced for lavas to deep-seated rocks, which is hardly feasible. The Skaergaard intrusion can in this connection not be accepted as a typically "deep-seated" rock; rather it represents a crystallization in a volcanic neck.

Buddington et al. then discuss the probable temperature of granitic and rhyolitic magma. They base their estimates upon the well known work of Bowen and Tuttle (1950) on the system of the alkali feldspars and on the work by the same authors (1951) on the melting temperatures of mixtures of $NaAlSi_3O_8$, $KAlSi_3O_8$, and SiO_2 under varying water vapor pressures. Thus they assume, on the basis of the composition of the alkali feldspar found in the alaskite in the Adirondacks, that this rock originally crystallized at a temperature above 660°C. I do not consider this an improbable temperature, but in arriving at this figure they have postulated that the microperthite in the alaskite developed by exsolution of an originally homogeneous crystal. Again I will not deny this possibility, but I should like to point out that these perthites may well have been formed through replacement of plagioclase by potash feldspar. A formation of such perthites in this way has been proposed by me (1955). If this is the case for the microperthites in the alaskite, the temperature for their formation cannot be deduced in the way done by the authors.

Buddington et al. consider the temperature of granitic and rhyolitic magmas to be between 675°-850°C. Generally speaking, this may be so for ordinary granitic magmas but is not necessarily correct for magmas of charnockitic affinities. The alaskite is characterized by very small amounts of dark minerals, but other rocks of granitic affinities in similar milieus contain appreciable amounts of rhombic pyroxene. (The birkremite in the Egersund

area of Norway may be classified as a pyroxene-rich alaskite.) This shows the magma to have been "dry." If then, as we must believe, the charnockitic magma in the deeper part of the Earth (6-11 miles as estimated by Rosenqvist (1952), which is cited by Buddington et al.) is dry, it will hardly melt much below 950°C, which is the melting point of a dry granitic magma at 1 atmosphere pressure (Goranson, 1938).

Thus I believe that granites formed under the conditions of granulite facies between 700°-800°C *cannot have crystallized from a magma.* The same conclusion is reached by Ramberg (1949) from other considerations.

The temperature derived by Buddington et al. for the injected granite pegmatites, 550°-700°C, is just a personal opinion. It is well known that pegmatites as well as granites start to form in the middle of epidote amphibolite facies. According to Ramberg (1952) and Rosenqvist (1952), whom Buddington et al. mention in their discussions of metamorphic temperatures, this corresponds to a temperature below 400°C. As there is no certain way of telling an "injected" pegmatite from one formed by metasomatism, we cannot assume generally that the temperature of formation is between 550°-700°C.

The temperature derived by Buddington et al. for the metamorphic "granulites" in the Adirondacks is based upon estimations by Ramberg and Rosenqvist and is, as far as one can guess, closely correct. They do, however, consider the alaskite to have formed at slightly higher temperatures than the other rocks in granulite facies within the area. The reason for this assumption is not quite clear, but it seems to be based upon the less microperthitic nature of the alkali feldspar of the latter. This, however, is hardly a proof. The diagrams of Bowen and Tuttle indicate the *minimum* temperature of formation of a certain alkali feldspar and do not state anything about the maximum temperature. For an exact comparison between the two we have to consider the coefficient of distribution of sodium in coexisting orthoclase and plagioclase (Barth, 1951). If the plagioclase is more basic in the "granulites" than in the alaskite, the coefficient of distribution in the two rocks may show the same value, thereby indicating the same temperature of formation.

I have tried above to show how the estimates of the temperature of formation for the different rock types given by Buddington et al. may be in error. This seriously affects their conclusions about using the content of TiO_2 in magnetite as a geological thermometer. Furthermore, I have tried to show that the estimated temperatures, especially for the felsic rocks, *probably do not correspond to magmatic temperatures at those depths.*

So much for the temperature of formation of the different host rocks. What is, at least in my opinion, much more important, is whether the content of TiO_2 in magnetites can be used as a relative geological thermometer for rocks of different kinds regardless of their mode of formation and degree of oxidation. The answer to this, I am convinced, must be no.

By crystal-chemical reasoning it can be stated that the general tendency is for the high temperature magnetites to be relatively enriched in titanium. Ulvöspinel, and most probably also magnetite, has the spinel structure with

"variate atom equipoints." Thus we can write the isomorphous series magnetite-ulvöspinel as follows:

Coordination number	Magnetite		Ulvöspinel
(4)	$Fe^{\cdot\cdot\cdot}$		$Fe^{\cdot\cdot}$
(6)	$Fe^{\cdot\cdot}$	$\rightleftarrows$	$Ti^{\cdot\cdot\cdot\cdot}$
(6)	$Fe^{\cdot\cdot\cdot}$		$Fe^{\cdot\cdot}$

There are two things indicating that high temperature will favor the formation of ulvöspinel: It exhibits a disorder structure and, as shown by Buerger (1948), atoms are able to assume lower coordination numbers when the temperature increases. Bivalent iron in tetrahedral coordination requires high temperature (for example, iron in the slag mineral iron-åkermannite). High pressures will probably have the opposite effect. Buddington et al. seem to disregard completely the pressure effect on the mix-crystal formation. This is dangerous, especially since they are directly comparing temperatures deduced from the content of TiO_2 in magnetite in lavas with temperatures of granulite facies rocks.

The authors believe that the bulk composition of the rock, though possibly a modifying factor, is not important in controlling the composition of the titaniferous magnetite. Accordingly they interpret the general lowering of the TiO_2 content in magnetites in passing from a gabbroic rock to a granitic as an effect of temperature only. It is well known that Ti is enriched in basic rocks. This, however, has nothing to do with the temperature of formation of the rocks. In passing from gabbroic to granitic rocks the ratio Fe_2O_3:FeO usually increases, thus influencing the chemical composition (including the distribution of TiO_2) of the minerals concerned. As substitution by Ti in the magnetite lattice is followed by a reduction of some of the trivalent iron, it is obvious that an oxidizing milieu will tend to oppose this. Thus it is surprising when Buddington via private letter states that the degree of oxidation varies widely in the granitic rocks while the data composition of the magnetite show no detectable variation.

On page 500 in their paper Buddington et al. state that in metamorphic reconstitution at successively lower temperatures the total composition remains constant while the composition of the titaniferous magnetite changes. Accordingly they ascribe these changes to temperature. However, a metamorphic reconstitution is apt to be followed by a difference in the FeO-Fe_2O_3 ratio. Furthermore, new silicate minerals are formed inducing a change in the relative proportions of the metal oxides available for the oxide formation. (TiO_2 is relatively enriched in the low metamorphic femic minerals: Ramberg, 1948.)

A glance at the hypothetical phase diagram FeO-Fe_2O_3-TiO_2 (fig. 1) suffices to demonstrate that not temperature but the chemical composition of the system (i.e. the relative amounts of metal oxides available for the oxide formation) is the dominant factor in controlling the amount of Ti in magnetite. Extensive solid solution exists at high temperatures only between the pairs magnetite-ulvöspinel and ilmenite-hematite. At successively lower tem-

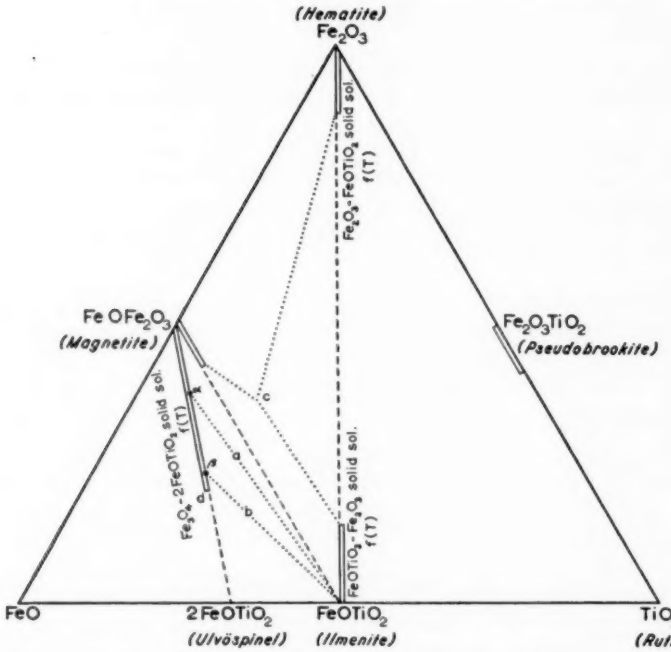

Fig. 1. Hypothetical and simplified phase diagram for the system FeO—Fe_2O_3—TiO_2. The diagram is thought to be valid at one temperature only, and the possible areas of solid solution believed to exist at this temperature are indicated.

peratures an immiscibility gap develops. Extensive solid solution between magnetite and ilmenite is for structural reasons hard to believe. (Exsolved ilmenite lamellae in magnetite are no proof of an original magnetite-ilmenite mix-crystal, Vincent and Phillips, 1954).

A solution of a composition represented by "a" in figure 1 is supposed to yield an ilmenite as primary crystallization phase. When the liquid arrives at the boundary line separating ilmenite from magnetite, a mix-crystal of composition in the neighborhood of "α" will crystallize in equilibrium with the ilmenite phase. The maximum content of "ulvöspinel in magnetite" at this temperature is represented by "d". If the titanium content in magnetite could be used as a geological thermometer the mix-crystal "α" obviously has to change its composition towards "d". Chemically this can best be expressed by the equation:

$$\underset{\text{Mt.}}{FeO.Fe_2O_3} + \underset{\text{Il.}}{FeO.TiO_2} \rightleftarrows \underset{\text{U.}}{2FeO.TiO_2} + \underset{\text{H.}}{Fe_2O_3}$$

This reaction is strongly dependent on the oxygen pressure. This is even more evident if the equation is written:

$$2(FeO.Fe_2O_3) + 6(FeO.TiO_2) \rightleftarrows 6(2FeO.TiO_2) + O_2$$
$$\text{Mt.} \qquad \text{Il.} \qquad \text{U.}$$

The oxygen pressure is a measure of the degree of oxidation, which in our system will be indicated by the $FeO:Fe_2O_3$ ratio (i.e. the chemical composition).

Initial compositions at a, b, or c, indicated on figure 1, must thus be believed to give magnetites with different titanium contents though all in equilibrium with ilmenite.

The views expressed above support the theory of Foslie that an excess of Fe_2O_3 in the magma will prevent the magnetite-ulvöspinel mix-crystal formation.

Thus it can be concluded: Although there is a general tendency for high temperature magnetites to be high in titanium, this can by no means be used as a geological thermometer. Actually the chemical compositions of the ore-forming solutions influences, more directly and more radically, the titanium content of magnetites.

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REPLY

A. F. BUDDINGTON

All the points raised by Heier were recognized and taken into consideration in the original paper. The estimated temperatures were stated only to $\pm 100°C$ to allow for the effect of pressure, subordinate variations in composition of system and the oxide minerals, etc. No new experimental or theoretical quantitative data are offered by Heier, and I see no need at this time to revise the original estimates or their implications.

The effect of pressure at a depth of 10 km in raising melting intervals may be offset by the equivalent counter-effect of only about one percent of additional H_2O which lowers the melting interval. The probable differences of temperature between lavas and equivalent plutonic magmas were all taken into account and correlated with a systematic difference in the composition of the magnetite in the equivalent rocks.

We have a large amount of data which for reasons of space were not included in the original paper. A selection from these data is presented below to amplify further the discussion (table 1).

Heier suggests that pyroxene granites as distinguished from hornblende or biotite granites should form at higher temperatures. I agree, and the data show that this *is* the usual relationship.

Heier emphasizes the importance of oxidation in controlling the mineral assemblages for the iron and titanium oxides. Again I agree but differ on the interpretation of the results. The degree of oxidation varies widely in the granitic rocks so that with the same total amount of Fe and Ti in the rock the mafic minerals may be femaghastingsite plus ilmenite and magnetite with the latter predominant, or ferrohastingsite plus ilmenite alone. The composition of the magnetite, however, shows no systematic variation within the sensitivity of our data. There is an analogous relationship in the pyroxene syenitic gneisses. Again, the magnetite of the pyroxenic gneisses of the granulite facies has 2.6-5.1 percent TiO_2, and ilmenite is the associated mineral. The magnetite of the gabbroic anorthosite and anorthositic gneisses of the granulite facies has 3.0-4.1 percent TiO_2, and the associated minerals are titanhematite and ilmenite. The granulite facies has the more oxidized assemblage, but the range of variation in composition of the magnetite for each is similar. Recent data of Akimoto (1955) show that the iron-titanium oxide mineral of certain volcanics may be a solid solution with up to 43 percent Fe_2O_3, and yet the associated magnetite may have substantial $2FeO.TiO_2$ in solid solution. The application of the Foslie principle that excess Fe_2O_3 will prevent the formation of $2FeO.TiO_2$ obviously is not a simple matter.

Heier's discussion of the system Fe_3O_4—$2FeO.TiO_2$—$FeO.TiO_2$ is not directly relevant, because the curve for the variation in TiO_2 content of magnetite with temperature roughly sketched as figure 2 in the original paper is based only on rocks in which the two significant and predominant iron and titanium oxide minerals are ilmenomagnetite and ilmenite, each occurring as separate distinct grains.

TABLE 1

Composition of Titaniferous Magnetite Approximating Ilmenite-Magnetite System from Different Kinds of Rocks*

	Fe_3O_4	$FeO.TiO_2$	Fe_2O_3	$2FeO.TiO_2$
Diabase pegmatite, Gettysburg sill, Pennsylvania	49.8	40.1	10.1	—
Tholeitic basalt, Okata, Osima, Japan	60.4	33.3	6.3	—
Fayalite ferrogabbro, Skaergaard	59.1	38.0	2.9	—
Gabbro, Skaergaard	63.1	28.2	—	8.7
Diabase, Gettysburg sill, Pennsylvania	67.8	32.2	—	—
Diabase, Gettysburg sill, Pennsylvania	66.8	32.0		1.2
Groundmass of andesite, Hatazyuku, Hakone, Japan	64.6	26.7	8.7	—
Pyroxene-ilvaite granophyre, Gettysburg sill, Pennsylvania	77.7	18.2	—	4.1
High grade ilmenomagnetite-ilmenite ore in anorthosite, Tahawus, Adirondacks	78.7	17.2	4.1	—
High grade ilmenomagnetite-ilmenite ore in anorthosite, Tahawus, Adirondacks	71.0	24.6	4.4	—
Phenocryst hornblende andesite, Minamihashimura, Oita, Japan	64.0	21.3	14.7	—
Phenocryst, hornblende dacite, Japan	82.0	18.0	—	—
Augite microperthite quartz syenite, Whitestone farm, Adirondacks	84.9	13.4	—	1.7
Hedenbergite microperthite granite, New Jersey	83.3	15.3	—	1.4
Fayalite-ferrohedenbergite microperthite granite, Adirondacks	80.3	16.9	2.8	—
Hornblende microperthite granite, average for 6, NW Adirondacks	83.4	10.4	6.2	—
Biotite-fluorite microperthite alaskite, average for 6, NW Adirondacks	86.5	8.1	5.4	—
Pyroxene and pyroxene-hornblende garnetiferous microperthitic microcline granite gneiss, granulite facies, average for 4, NW Adirondacks	90.3	7.0	2.7	—
Hornblende oligoclase-microperthitic microcline granite gneiss, upper range of amphibolite facies, average for 5, NW Adirondacks	86.6	6.8	6.6	—
Average for 4 members of charnockitic series, Madras, India (Howie, 1955)	94.6	5.4	—	—
Hornblende oligoclase-microcline granite gneiss with sphene, lower range of amphibolite facies, average for 5, NW Adirondacks	95.8	2.0	2.2	—

* Recalculated in terms of 100 weight percent Fe_3O_4 + $FeO.TiO_2$ + Fe_2O_3 + $2FeO.TiO_2$.

Note: Reference for data on Japan rocks is Akimoto (1955), and for the Skaergaard rocks, Vincent and Phillips (1954). All the Adirondack samples have been examined with the microscope; the titaniferous magnetite is ilmenomagnetite; ilmenite is present as independent grains, and martite is insignificant. All analyses, other than those quoted, are by Joseph Fahey and Angelina Vlisidis.

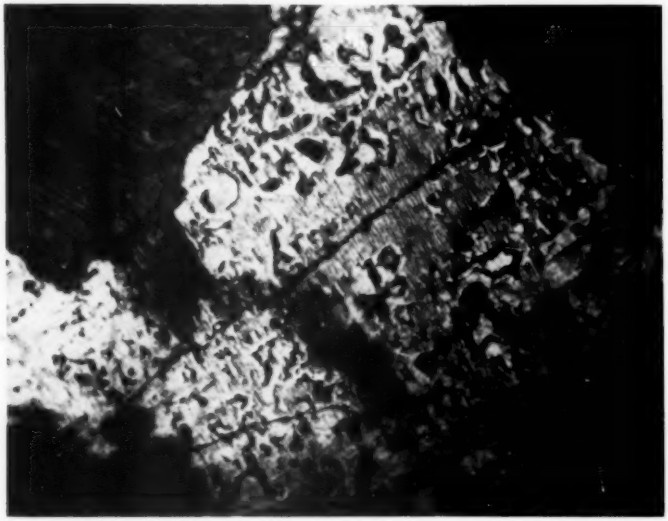
It appears to be true that in many, perhaps most rocks (hornblende and biotite granites and equivalent gneisses excepted), the titaniferous magnetite does have substantial quantities of $2FeO.TiO_2$. However, there are numerous rocks varying in composition from mafic to salic whose titaniferous magnetite belongs essentially to the system Fe_3O_4—$FeO.TiO_2$ with minor amounts of other oxides. Data for a series of such rocks are given in the accompanying table. The available data are not adequate to discuss how the percent of TiO_2 in magnetite in rocks varies for rocks of otherwise similar composition depending on whether $2FeO.TiO_2$ or $FeO.TiO_2$ is the predominant iron-titanium oxide. It is puzzling that with as much data as there are some systematic difference has not been obvious. It may yet prove that the Fe_3O_4—$2FeO.TiO_2$—$FeO.TiO_2$ system does have some use in geologic thermometry when we know more about it.

I should have been more explicit in the original paper in emphasizing that general correlation of the titanium content of magnetite with the nature of the rock was one problem and its use as a geological thermometer was another.

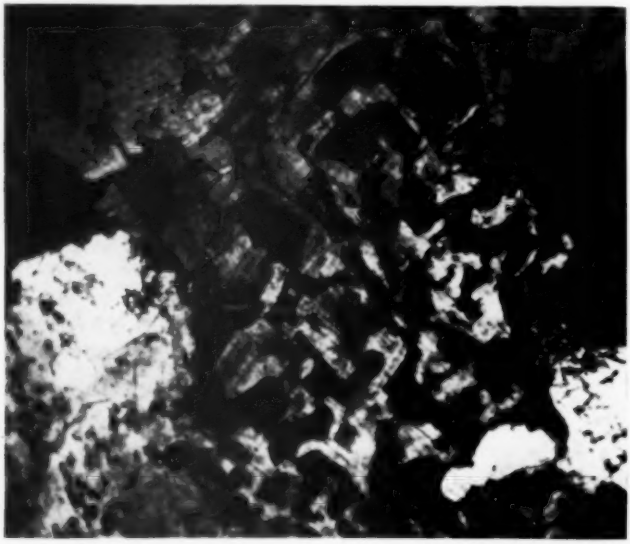
Heier's discussion emphasizes the composition of the total rock as controlling the composition of the magnetite. I cite in answer these examples: An unmetamorphosed rich ore specimen from a body in anorthosite with pyroxene and labradorite gangue from Tahawus in the Adirondacks carries 19.4 percent TiO_2 and the ilmeno-magnetite 13.0 percent TiO_2, whereas a metamorphosed sample of ore with clinochlore gangue from Otanmaki, Finland carries 18.86 percent TiO_2 and the magnetite only 2.5 percent TiO_2. The magnetite of the average hornblende microperthite granite may also be compared with that in the equivalent oligoclase-microperthitic microcline granite gneiss recrystallized in the upper part of the amphibolite facies. The total Fe and Ti in both are similar, and ilmenite as separate grains is present in both. Yet the magnetite of the granite has about 1.5 times as much $FeO.TiO_2$ as the gneiss. Many other examples could be cited.

Heier states that exsolved ilmenite lamellae in magnetite are no proof of an original magnetite-ilmenite mix-crystal and that similarly microperthites (mesoperthites) with the two feldspars in similar amounts and in normal intergrowth need not be due to exsolution but may be due to replacement. I can only say that I have yet to see any paper which provided conclusive proof that such normal intergrowths of the type under discussion of either pair of these minerals are not of exsolution origin. Heier in the paper cited (1955) by him shows several photomicrographs purporting to show "convincing" evidence that a relatively coarse replacement-like antiperthite (nos. 1 and 2) is an intermediate stage in the replacement of plagioclase by potash feldspar to form a normal antiperthite (nos. 3-5). The pictures alone suggest to me just the opposite interpretation, namely, that the replacement-like antiperthite is the result of modification and coarser recrystallization of the normal antiperthite and that the latter is, as usual, due to exsolution. (See plate 1, A and B for comparison.) Heier states that "extensive solid solution between magnetite and ilmenite is hard to believe." To save elaboration here I refer to a recent review by Nicholls (1955, p. 144-152) of this matter. The probable

PLATE 1



A. Antiperthite (mesoperthite) core of exsolution origin with incipient recrystallization. Outer zone of feldspar is recrystallized with segregation of microcline (black) and some addition of albite. × 53 diameters, Hibernia mine, New Jersey.



B. Antiperthite with appearance as though microcline (black) was replacement of twinned sodic plagioclase. Gradation with A shows that actual origin is one of recrystallization and redistribution in presence of sodic solutions. × 53 diameters, Hibernia mine, New Jersey.

Chemical analyses show that the rocks of A and B carry a slightly higher ratio of Na_2O to K_2O than rock with primary antiperthite only.

form of the phase diagram proposed by him shows the potentiality for *extensive* solid solution of $FeO.TiO_2$ in Fe_3O_4. Ramdohr (1955, p. 153) also cites ilmenite of exsolution origin in magnetite up to equal parts.

It was specifically stated by us that the composition of the system in which the titaniferous magnetite formed may be a modifying factor, but it is *still* maintained that the temperature control is so much a greater factor where the iron-titanium oxides are essentially ilmenite and titaniferous magnetite that the composition of magnetite belonging essentially to the system Fe_3O_4—$FeO.TiO_2$ *does* have value as an approximate geologic thermometer if the usual discretion necessary in most petrological interpretations is exercised.

In connection with another matter, namely, the potentiality of the iron-titanium oxides forming part of an immiscible magma under certain conditions, reference should have been made in our paper to an article by Per Geijer (1935) in which he argued in favor of an origin by immiscibility for certain apatite-bearing iron ores such as the Kiruna type.

The opposing views of Heier will serve to point up where *data* are needed. The problem is probably at least as complex as that of the feldspars which is requiring so many man years of research.

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REVIEWS

Fundamental Formulas of Physics; DONALD H. MENZEL, editor. P. xxxv, 765. New York, 1955 (Prentice-Hall, $13.50).—To every research worker in physics, this should prove a most useful volume. It contains more than the bare bones of formulas; it endeavors to make them meaningful by including the definitions of terms and often the context which allows a reader to see the origin and the purpose of mathematical equations. The list of authors is very impressive—rarely has so much talent been brought to bear upon a book of formulas. On the whole, all fields of physics are treated with adequate emphasis. A few peculiarities do occur: it is a little hard to justify, perhaps, that 44 pages should be devoted to geometrical optics, 24 to physical optics, and only 20 to quantum mechanics. Nevertheless it is noteworthy that in a book with 27 authors there should be so remarkable a measure of correlation between parts.

HENRY MARGENAU

Nuclear Geology; HENRY FAUL, editor. P. xvii, 414; 84 figs. New York, 1954 (John Wiley & Sons, $7.00).—*Nuclear Geology*, subtitled "A Symposium on Nuclear Phenomena in Earth Sciences," is a welcome addition to the new and fast-growing science of nuclear or isotope geology, sometimes also called nuclear geophysics.

Twenty-six leading workers in this new science have contributed articles. The topics covered include "Fundamental considerations, instruments, and techniques of detection and measurement," "Uranium and thorium," "The abundance of potassium," "Rare gases and fission in nature," "Heat from radioactivity," "Radiation damage and energy storage," "Hydrocarbons formed by the effects of radioactivity and their role in the origin of petroleum," "Geophysical exploration by nuclear methods," "Determination of absolute age," and "The origin of Earth."

The book will be of great interest and use to both theoretical and practical geologists and to all interested in the age and origin of the Universe. With one notable exception the coverage is broad and authoritative. This exception is the lack of coverage of almost the whole field of the variations of stable isotopes in nature. The book is concerned almost entirely with the parent and daughter radioactive elements and associated geologic phenomena. Dr. Faul has explained in the preface of the book that of 53 scientists asked to contribute only 26 "actually came through." This explanation is satisfactory to the reviewer if there were no undue haste on the part of the editor to publish this first review book on the new science.

Many of the subjects discussed have been under intensive investigation for a decade or more. The science of the variations of stable isotopes in nature is a comparatively new science and is currently on much weaker theoretical (and, generally, analytical) grounds than the study of radioactive phenomena in nature. It is likely that most workers in the field of stable isotopes were reluctant to write reviews because of the general immature state of knowledge of this subject. It should be covered in the next edition of *Nuclear Geology*, for even in the past few years significant advances have been made. The study of stable isotopes will undoubtedly contribute as much to the understanding

of Earth science phenomena as has the study of radioactive phenomena in the past.

Dr. Faul has made a most useful contribution in his compilation of a table (table 9) of published radioactive age measurements. Obviously a great amount of editorial labor has gone into this complete work. The compiled ages range from recent (C^{14} dates) to the most ancient. For each "age" the mineral locality, author or analyst, method, and age are given. When possible, stated limits of error are given. It will be of extreme usefulness to critical readers, especially to those not directly engaged in the analytical problems of age determination, to be able to compare and evaluate the "ages" on given localities by a variety of different methods. Strikingly, ages are frequently not concordant.

For example, among the strongest evidences for placing the base of the Cambrian at 500 to 600 x 10^6 years ago are the analytically correct measurements made on the Güllhogen, Sweden, kolm. This is an upper Cambrian unit, accurately dated by index fossils. Its relatively high uranium content makes radioactive age measurements possible. The Pb^{206}/U^{238} age is 388 x 10^6 years, and the Pb^{207}/Pb^{206} age (considered generally most trustworthy of the U-Th-Pb methods) is 770 x 10^6 years. However, F. Wickman recalculated the age as 442 x 10^6 years on the theoretical basis of radon loss. Unfortunately, these facts are not generally known to geologists and cosmologists.

Another most interesting compilation of ages by various methods is made on the Katanga rocks. These ages are highly concordant and with two exceptions (out of 22 listed measurements) 582 to 676 x 10^6 years. But in this case the evidence that these rocks are Precambrian (as assumed by those who put the base of the Cambrian at about 600 x 10^6 years) is not conclusive.

The two examples given should serve to emphasize the usefulness of this table of measured ages, but it should also point out the need for a highly critical review chapter on the geological *meaning* of age measurements and the present state of analytical comparison of the methods. Of course, even since the publication of *Nuclear Geology*, notable advances have been made in comparing the analytical methods by several competent laboratories. Several of the individual review articles include highly self-critical analyses of the sources of error.

The Historical Introduction, written by Dr. J. P. Marble, is a readable, factual, and interesting review of the historical development of the science of nuclear geology.

No real review discussion of the age of meteorites and the cosmic time scale is included. The concluding chapter, "The Origin of the Earth," by Professor Harold C. Urey, is evidently an attempt at setting the cosmic background for the subject matter of *Nuclear Geology*, but the article is out of context and fails in its aim. Professor Urey, with his background and accomplishments in both radioactive and stable isotope geology, is capable of writing such a background review article, and it is hoped that he will do so for the next edition of *Nuclear Geology*.

In conclusion, the reviewer feels that *Nuclear Geology* will be a valuable addition to any library. Dr. Faul has made much of the subject matter of this

new science readily available to new students and workers in the field and this book will have a healthfully stimulating influence on geology and cosmology.

WILLIAM H. PINSON, JR.

Atlas of Paleogeographic Maps of North America; by CHARLES SCHUCHERT. P. xi, 177; 84 maps. New York, 1955 (John Wiley & Sons, $4.75).—The Atlas, edited by Dr. Carl O. Dunbar, Director of the Peabody Museum of Natural History of Yale University, consists of an Introduction by Dunbar and 84 maps of North America which show the relations of land and sea through geologic time from the lowest Cambrian to and including the Pleistocene. Each map has a title page on which are given the stratigraphic units on the distribution of which the map is based. Problems of correlation were fundamental to the construction of each map, and on the title pages of some maps there is a note by Dunbar calling attention to some changes of opinion with respect to some stratigraphic units.

The number of maps assigned to each period of geologic time is as follows:

Period	Number of Maps
Cenozoic	8
Cretaceous	7
Jurassic	5
Triassic	6
Permian	4
Pennsylvanian	6
Mississippian	10
Devonian	8
Silurian	10
Ordovician	12
Cambrian	8
Total	84

The maps, as published, are at the stage of completion as drawn in pencil by Professor Schuchert at the time of his death in October, 1942. There were additional maps, and those published were selected by Dr. Dunbar and Miss Clara M. LeVene. These were redrawn on a new base map by Dr. John E. Sanders. Data discovered subsequently to Prof. Schuchert's death have not been used to change any map. There is obviously need for some modification, but to the reviewer it does not seem likely that the new data would greatly modify any map.

No one was more aware than Schuchert that each map represents a considerable span of time, and hence it is probable that few show exact relations of land and sea at any one time of the span represented. Each map shows the greatest extent of the sea during the span. Schuchert fully realized this, and in his hope to approximate greater accuracy as his studies developed, the number of maps increased from the 49 published in 1910 in his *Paleogeography on North America* to the 84 published in the present study. It needs to be stated, however, that at the time of his death in 1942 there were additional

maps in various stages of completion. Schuchert fully appreciated that each map would serve as a sort of signpost.

Only those who have followed the development of paleogeographic maps can appreciate the tremendous time and labor involved. Each stratigraphic unit must be evaluated and correlated; this is always fraught with possibility of error. The reviewer was a student of Professor Schuchert's when he began his early studies of paleogeography and he saw something of their development. Paraphrasing Longfellow's *Village Blacksmith*:

"From early morn til evening late
He fixed the bounds of lands and seas."

And this work extended over more than a third of a century.

The maps are storehouses of information. A glance at any one readily shows the positions of marine waters for the span of time represented and the areas of existing and possible exposures and occurrences of the stratigraphic units formed during the span of time represented. Every geologist would do well to have a copy of the *Atlas* or at least have access thereto. It is regrettable that publication had to be delayed for more than a decade after the author's death as thereby more than a decade of usefulness has been lost.

W. H. TWENHOFEL

Stratigraphic Geology; by MAURICE GIGNOUX, translated by GWENDOLYN G. WOODFORD. P. xvi, 682; 155 figs. San Francisco, 1955 (W. H. Freeman and Company, $9.50).—Inasmuch as nearly all major units of the geologic time scale were first named in Europe, an understanding of the relationships of these standard sections is essential to anyone who would delve far into problems of stratigraphic geology. With the publication of this excellent, accurate, and readable translation of Gignoux's well known summary of European stratigraphy, American students should readily be able to learn not only the stratigraphic details of these classic areas but also the essential features of their structural setting and the geologic history of the European continent. *Géologie Stratigraphique* was first published in 1925; the fourth edition, of which this is a translation, appeared in 1950 and is thoroughly up to date. It is by far the best organized and clearest account of European geology to be found in any language. The brevity of references to other continents in no way detracts from its value as a reference on Europe.

Mrs. Woodford is to be congratulated upon the clarity of the translation and its careful adherence to the original. The publishers have done excellent work in producing a legible book, using large type, and even redrafting the maps and diagrams to insert English names for localities and rock units.

No better reference can be recommended to anyone who needs information on European geology.

JOSEPH T. GREGORY

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R. T. Chew III ($ .40). 1009-J, Criteria for Outlining Areas Favorable for Uranium Deposits in parts of Colorado and Utah; by E. J. McKay ($ .55). 1015-D, Chromite Deposits in Central Part, Stillwater Complex, Sweet Grass County, Montana; by A. L. Howland ($1.50). 1015-I, Geology of the Dry Valley Quadrangle, Idaho; by E. R. Cressman and R. A. Gulbrandsen ($1.00). 1017, Lead-zinc Deposits of Cordillera Blanca and Northern Cordillera Huayhuash, Peru; by A. J. Bodenlos and G. E. Ericksen ($2.75). 1021-B, Hawaiian Volcanoes During 1952; by G. A. MacDonald ($ .55). 1021-C, Geologic and Airborne Radioactivity Studies in the Rock Corral Area San Bernardino County, California; by R. M. Moxham, G. W. Walker, and L. H. Baumgardner ($1.00). 1021-D, Hawaiian Volcanoes During 1953; by G. A. MacDonald and J. P. Eaton ($ .20). 1024-B, Investigations for Radioactive Deposits in Southeastern Alaska; by W. S. West and P. D. Benson ($ .55). 1024-C, Radioactivity Investigations at Ear Mountain Seward Peninsula, Alaska, 1954; by P. L. Killeen and R. J. Ordway ($ .40). 1025, Bibliography of North American Geology, 1951; by R. R. King, V. M. Jussen, J. S. Pomeroy, and V. L. Skitsky ($1.25). 1027-C, Dolomite Deposit Near Marble, Stevens County, Washington; by Charles Deiss ($ .60). 1027-G, Lithium Resources of North America; by J. J. Norton and D. M. Schligel; ($ .15). 1031-A, Geologic Investigations of Proposed Power Sites at Cooper, Grant, Ptarmigan, and Crescent Lakes, Alaska; by George Plafker ($1.00). 1032-A, Uranium Deposits in the Eureka Gulch Area, Central City District, Gilpin County, Colorado; by P. K. Sims, F. W. Osterwald, and E. W. Tooker ($ .50). 1033-B, Geophysical Abstracts 161, April-June 1955; by M. C. Rabbitt, D. B. Vitaliano, S. T. Vesselowsky et al.; ($ .25). 1033-C, Geophysical Abstracts 162, July-September 1955; by M. C. Rabbitt, D. B. Vitaliano, S. T. Vesselowsky et al. ($ .25). 1036-B, Rapid Determination of Germanium in Coal, Soil, and Rock; by Hy Almond, H. E. Crowe, and C. E. Thompson ($ .15).

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